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THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

" Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Monit. Polit.* lib. i. cap. 1.

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THE

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PHILOSOPHICAL MAGAZINE.

I. *Analysis of the Chromate of Iron from the Ouralian Mountains in Siberia.* By A. Laugier*.

M. Pontier, in a tour made through the department of Var, in the year 7, found, near La Bastide de la Cassade, a mineral, which he sent to the Council of Mines under the name of *blende*, and which M. Tassaert first discovered to be a combination of chromic acid and oxide of iron.

His opinion was afterwards confirmed by M. Vauquelin in the tenth volume of the *Journal de Mines*; where he at the same time announced the proportions of chrome and iron, together with the presence of alumine and silica.

M. Meder has since found in Siberia, in the Ouralian mountains, on the banks of the Wiasga, a substance very similar to the mineral of Var. A specimen of this having been given to me by M. Steinacher, member of the corporation of apothecaries of Paris, who had received it from count Mussin Puschkin, counsellor of the mines of Russia, I conceived it might be useful to examine it, and compare the results of my examination with the analysis of the mineral of Var published by M. Vauquelin, fully persuaded that my labours would be rewarded with some advantage.

Physical Properties.

Although the mineral of Siberia be very similar in appearance to that of Var, an atttentive examination of it would lead us to suspect, that in the first of these the metal is purer and more abundant than in the second; its

* From *Annales de Museum d'Histoire Naturelle*, No. 35.

fracture is lamellar, and not granulated; its metallic brilliancy is more vivid, and it evidently contains a smaller admixture of earthy matters. The specimen in my possession shows spots of green on some parts of its surface, which are discovered to be oxide of chrome. The specific gravity of the two also serves to support my conjecture regarding them. That of my specimen is 4·0579, while the specific gravity of the mineral of Var is only 4·0326. This difference in weight necessarily makes a difference in the proportions of the metallic part of the two chromates; and, as we shall presently see, the analysis agrees completely with this physical property.

Chemical Examination.

1. When strongly calcined it loses about 1-100th part of its weight, and acquires a reddish brown colour. Having placed 100 parts of this mineral reduced to an impalpable powder, together with 300 parts of pure potash, in a crucible of platina, I exposed the whole to a strong heat. The mass, when removed from the fire, and nearly cooled, was in part of a green and in part of an orange colour. The water which I added to it assumed a rich citron yellow, exactly similar to the colour of chromate of potash. When the mass no longer gave a tinge to distilled water, I digested it in very weak muriatic acid, with the intention of separating what part of the oxide of iron had been set free by the action of the potash, without touching, in any degree, the chromate of iron, which was still undecomposed. I now washed the remaining substance again, till the water came off insipid, and a second time melted it with a portion of pure potash.

In this way I heated the mineral six different times, alternately with potash and muriatic acid; setting aside the alkaline and acid solutions, that I might examine them separately.

The residuum consisted of a brownish gray matter, weighing 0·90: muriatic acid took up from it a little iron, and the remainder was completely dissolved in nitro-muriatic acid.

acid. This solution, which was of a reddish brown colour, let fall a yellow precipitate on the addition of muriate of ammonia, and a deep red precipitate by the muriate of tin at the minimum of oxidation: it suffered no precipitation by prussiate of potash, and formed with soda a triple salt of a beautiful red colour. It was therefore no other than a portion of platina which had been separated from the crucible by the six successive manipulations; and there was every reason to believe that the alkali and acid had completely dissolved the constituent ingredients of the mineral in question.

Examination of the Alkaline Solution.

2. This solution had a very beautiful deep yellow colour. The last portions of the solution had a greenish tinge, which disappeared on the spontaneous deposition of a brown substance in small quantity, which I discovered to be oxide of manganese. I poured, by degrees, into the solution, evaporated to one-half its former bulk, such a quantity of nitric acid as saturated the caustic portion of alkali; and a precipitate fell to the bottom, which I collected upon a filter, then washed it, and submitted it to calcination. It weighed 11 hundredth parts, and was set aside for future examination.

The addition of a slight excess of nitric acid produced no precipitation but merely a violent effervescence, and caused the solution to assume a very deep orange red colour. When evaporated to dryness it left a saline residuum of a fine yellow colour, which dissolved completely in water: the solution, when acidulated with nitric acid, furnished by the nitrate of mercury a red precipitate, which after being dried in the air weighed 430 parts; and these by calcination were reduced to 52 hundredth parts of an oxide of chrome, having a beautiful green colour.

3. The matter precipitated in No. 2 by the nitric acid was melted with three parts of caustic potash; and the mass, when mixed with water, dissolved completely in muriatic acid. The solution, when evaporated to dryness,

left a residue, a part of which was insoluble in water, and weighed, after calcination, only one-half part: it consisted of a mixture of silica and oxide of iron. I now poured into the water which contained the soluble portion of the residuum a quantity of ammonia; and a white flocculent precipitate appeared, which formed with the water a jelly, and exhibited all the characters of alumine: it weighed 10 hundredth parts and a half.

From these experiments on the alkaline solution we may conclude that it contained chrome, alumine, a little oxide of manganese, together with some particles of iron and silica.

Examination of the Muriatic Solution.

4. The colour of this solution, which contained an excess of acid, was reddish yellow. On the addition of a solution of caustic potash a flocculent matter of a brownish red separated, which, when washed and mixed with the small quantity of oxide of iron already obtained, both from the alkaline solution and from the residue, discovered to be platina, weighed, after being calcined, 34 hundredth parts.

The solution continued still coloured after the separation of the oxide of iron, although the re-agents indicated the presence of no chrome: I was therefore convinced that it owed its tinge to a small quantity of platina. The excess of caustic potash which I had added contained some particles of alumine, and these I separated by means of muriate of ammonia.

5. With a view to ascertain the purity of the oxide of iron which had the usual colour and appearance of this oxide, I melted it again with pure potash: but water, when added to the mixture, received no tinge, and the re-agents indicated the presence of no foreign substance. We must therefore infer that the very weak muriatic acid which I made use of dissolved nothing but the oxide of iron.

From

From the above analysis it appears that the chromate of iron of Siberia contains in the 100 parts,

Oxide of chrome -	53
Oxide of iron - -	34
Alumine - - -	11
Silica - - -	1
	99
Traces of manganese and loss	1
	100

These results are nearly similar to what M. Vauquelin obtained from the chromate of Var.

Chromic acid - -	43
Oxide of iron - -	34·7
Alumine - - -	20·3
Silica - - -	2
	100

Does the chrome exist in the state of acid or in that of oxide in the mineral termed chromate of iron? M. Godon de Saint-Mesmin, in a memoir upon the combinations of chromic acid, read at the National Institute, has discussed this question, and is inclined to think that it is in the state of oxide. M. Vauquelin, in his report of this memoir, seems disposed to adopt the same opinion. I shall merely state, in support of this idea, an experiment which renders it probable. If the green oxide of chrome be gently calcined with pure potash, it is almost immediately brought to the state of an acid: we are therefore not warranted in asserting that chrome exists as an acid in the chromate of iron, while by the presence of potash this conversion of the oxide is rendered likely. It is then, at least, highly probable that the mineral which till now has received the name of chromate of iron, is, in fact, no other than a combination of the oxides of iron and chrome.

Since I finished my examination of the chromate of iron

of Siberia, I have learnt that M. Lowitz has also analysed this substance. I am ignorant of the exact proportions of its ingredients, according to his experiments, but if I may judge from the note relative to this subject which appeared in the *Journal de Physique*, his results were nearly the same with mine; for he announces that he found it to contain more than half its weight of oxide of chrome, together with iron, alumine, and a little silica.

II. *On the Tendency of Elastic Fluids to Diffusion through each other.* *By* JOHN DALTON*.

In an early period of pneumatic chemistry it was discovered that elastic fluids of different specific gravities being once diffused through each other, do not of themselves separate, by long standing, in such manner as that the heaviest is found in the lowest place; but, on the contrary, remain in a state of uniform and equal diffusion.

Dr. Priestley has given us a section on this subject (*vide* Experiments and Observations, &c. abridged, vol. ii. p. 441), in which he has proved the fact above mentioned in a satisfactory manner; and every one's experience since, as far as I know, has coincided with his conclusions. He has not offered any conjecture concerning the cause of this deviation from the law observed by inelastic fluids; but he suggests, that "if two kinds of air of very different specific gravities were put into the same vessel, with very great care, without the least agitation that might mix or blend them together, they might continue separate, as with the same care wine and water may be made to do."

The determination of this point, which seems at first view but a trivial one, is of considerable importance; as from it we may obtain a striking trait, either of the agreement or disagreement of elastic and inelastic fluids in their mutual action on each other.

It is therefore the subject of the following experiments to ascertain whether two elastic fluids, brought into contact,

* From *Memoirs of the Manchester Society*, second series, vol. i.

could

could intermix with each other independently of agitation. The result seems to give it in the affirmative beyond a doubt, contrary to the suggestion of Dr. Priestley, and establishes this remarkable fact, that a lighter elastic fluid cannot rest upon a heavier, as is the case with liquids; but they are constantly active in diffusing themselves through each other till an equilibrium is effected, and that without any regard to their specific gravity, except so far as it accelerates or retards the effect, according to circumstances.

The only apparatus found necessary was a few phials, and tubes with perforated corks: the tube mostly used was one 10 inches long, and of 1-20th of an inch bore; in some cases a tube of 30 inches in length and 1-3d inch bore was used: the phials held the gases that were subjects of experiment, and the tube formed the connection. In all cases the heavier gas was in the under phial, and the two were placed in a perpendicular position, and suffered to remain so during the experiment in a state of rest: thus circumstanced, it is evident that the effect of agitation was sufficiently guarded against; for, a tube almost capillary, and ten inches long, could not be instrumental in propagating an intermixture from a momentary commotion at the commencement of each experiment.

First Class.

Carbonic Acid Gas, with Atmospheric Air, Hydrogenous, Azotic, and Nitrous Gases.

1. A pint phial filled with carbonic acid gas, the 30 inch tube and an ounce phial, the tube and small phial being filled with common air, were used at first. In one hour the small phial was removed, and had acquired no sensible quantity of acid gas, as appeared from agitating lime water in it. In three hours it had the acid gas in great plenty, instantly making lime water milky. After this it was repeatedly removed in the space of half an hour, and never failed to exhibit signs of the acid gas. Things remaining just the same, the upper phial was filled with the different gases mentioned above repeatedly, and in half an hour there was always found acid sufficient to make the phial, one-half

one-half filled with lime water, quite milky. There was not any perceptible difference, whatever gas was in the upper phial *.

Second Class.

Hydrogenous Gas, with Atmospheric Air and Oxygenous Gas.

1. Two six-ounce phials were connected by the tube of a tobacco pipe three inches long, the upper containing hydrogenous gas, the lower atmospheric air: after standing two hours the lower phial was examined; the mixed gases it contained made six explosions in a small phial. The gas in the upper also exploded.

2. Two four-ounce phials connected with the ten-inch small tube stood two days, having common air and hydrogen gas. Upon examination the upper was found to be one-third common air by the test of nitrous gas. The gas in the under exploded smartly; that in the upper moderately, with a lambent flame.

3. Two one-ounce phials were connected by the ten-inch tube, containing common air and hydrogenous gas: in three hours and a half the upper was about one-third common air, and the under two-thirds; the former exploded faintly, the latter smartly.

4. Two one-ounce phials were connected as above; the under containing gas about three-fourths oxygenous, the upper hydrogenous: in three hours the latter was one-fifth oxygenous, and the former about one-half; the upper exploded violently, the under moderately.

5. Two one-ounce phials were again connected, the lower having atmospheric air, the upper hydrogenous gas; they stood fifteen hours, and were then examined: the upper gave 1·67 with nitrous gas, the under 1·66. Hence it is evident that an equilibrium had taken place, or the two gases were uniformly diffused through each other in both phials.

* The small tube of ten inches was then used, and a phial of common air; in one hour much acid gas had come through, as appeared by lime water.

Third

Third Class.

Nitrous Gas, with Oxygenous Gas, Atmospheric Air, Hydrogenous and Azotic Gases.

The results of the preceding experiments upon gases that have no known affinity for each other, were conformable to what, *à priori*, I had conceived; for, according to my hypothesis, every gas diffuses itself equably through any given space that may be assigned to it; and no other gas being in its way can prevent, though it may considerably retard, this diffusion. But in some of the following experiments, in which the two gases are known to have a chemical affinity for each other, I expected different results from what were found; perhaps without sufficient reason. For chemical union cannot take place till the particles are brought into contiguity; and the elastic force which sets them in motion appears, from the above experiments, to be a principle diametrically opposite to affinity. That circulation of elastic fluids, therefore, which we have now before us, cannot be accelerated by their having a chemical affinity for each other. Another circumstance deserves explanation: when nitrous and oxygenous gas are in the two phials, the residuary gases after the experiment are nearly as pure as before; because those portions of them that meet in the tube form nitrous acid vapour, which is absorbed by the moisture in the phials, and therefore does not contaminate either gas.

1. Two one-ounce phials were connected with the small tube, the under containing nitrous gas, the upper atmospheric air. After three hours the upper phial was taken off, when a quantity of air was perceived to enter, as was expected: the air in the upper phial was scarcely distinguishable from what it was at first; that in the under phial was still so much nitrous as to require its own bulk of common air to saturate it.

2. The above experiment was repeated, and the upper phial drawn off when the whole was under water, in order to prevent communication with the atmosphere. About one-sixth of an ounce of water entered the phials, to com-

pensate the diminution. Remaining air in the upper phial was a very little worse than common air; it being of the standard 1·47, when the former was 1·44. The gas in the under phial was still nitrous, and nearly of the same purity as at first; for three parts of it required four of atmospheric air to saturate them.

3. Nitrous gas and one two-thirds oxygenous were tried in the same way. After four hours the apparatus was taken down under water. The upper phial was two-thirds filled with water, and the gas in it was partly driven down the tube into the other phial, by which, and the previous process, the nitrous gas was completely saturated, and nothing but azotic, with a small portion of oxygenous, were found in the under phial. The remaining gas in the upper phial was still one-half oxygenous.

4. Nitrous gas and hydrogenous. In three hours the upper phial was one-fourth nitrous, and of course the under must have a like part of hydrogen.

5. Nitrous gas and azotic. After three hours the upper phial was one-fifth nitrous.

In the two last experiments the quantity of nitrous gas in the upper phial was less than might be expected; but the tube was at first filled with common air, and some must enter on connecting the apparatus, which is sufficient to account for the results.

Fourth Class.

Azotic Gas, with Mixtures containing Oxygenous Gas.

1. Azotic gas and one two-thirds oxygenous. After standing three hours the upper phial was of the standard 1·78, or about one-fourteenth oxygenous.

2. Azotic gas with atmospheric air. After standing three hours the upper phial was not sensibly diminished by nitrous gas; the under phial, however, had lost two per cent., or one-tenth of its oxygen. The reason of this was, that the azotic gas in this experiment, having been just made for it from nitrous gas, this last had not been completely saturated with atmospheric air, and hence had seized upon all the oxygen ascending into the upper phial.

Having

Having now related all the experiments I made of any importance to the subject, it will be proper to add, for the sake of those that may wish to repeat some of them, that great care must be taken to keep the inside of the tube dry; for if a drop of water interpose between the two gases, I have found that it effectually prevents the intercourse. Glass tubes should therefore be used, that one may be satisfied on this head, as the obstruction will then be visible.

I shall make no further comments on the above experiments by way of explanation; because, to those who understand my hypothesis of elastic fluids, they need none: and I think it would be in vain to attempt an explanation any other way. I cannot however, on this occasion, avoid adverting to some experiments of Dr. Priestley, which few modern philosophers can be unacquainted with: I mean those relating to the seeming conversion of water into air. (*Vide* Philos. Transact. vol. lxxiii. p. 414; or his Experiments abridged, vol. ii. p. 407.) He found that unglazed earthen retorts, containing a little moisture, when heated, admitted the external air to pass through their pores at the same time that aqueous vapour passed through the pores the contrary way, or outward; and that this last circumstance was necessary to the air's entrance. The retorts are air-tight, so far as that blowing into them discovers no pores; but when subjected to a greater pressure, as that of the atmosphere, or even one much short of it, they are not able to prevent the passage of elastic fluids. The fact of air passing into the retort through its pores, and vapour out of them at the same time, are elegantly and most convincingly shown by Dr. Priestley's experiments, in which he used the apparatus represented in Plate VII. fig. 1. of the edition above referred to. The doctor confesses his explanation of these remarkable facts is very inadequate; and no wonder, for it is impossible for him or any other to explain them on the commonly received principles of elastic fluids. But we will hear what he says on the subject:—"At present it is my opinion that the agent in this case is that principle which we call attraction of cohesion, or that power by which water is raised in capillary tubes. But in what manner it acts in this

this case, I am far from being able to explain. Much less can I imagine how air should pass one way and vapour the other in the same pores, and how the transmission of the one should be necessary to the transmission of the other. I am satisfied, however, that it is by means of such pores as air may be forced through, that this curious process is performed; because the experiment never succeeds but in such vessels as, by the air-pump at least, appear to be porous, though in all such."

The truth is, these facts, so difficult to explain, are exactly similar to those which are the subject of this memoir; only instead of a great number of pores, we have one of sensible magnitude—the bore of the tube. Let the porous retort have the same elastic fluid within and without, in the one case; and the two phials contain the same elastic fluid in the other, then no transmission is observable in either: but if the retort have common air, or any other gas, without, and aqueous vapour, or any other elastic fluid, except the outside one, within; then the motion in and out commences, just as with the phials in similar circumstances. In fact, this last observation has since been verified by Dr. Priestley himself, of which an account is given in No. 2, of the American Philosophical Transactions, vol. v. After alluding to his experiments above mentioned, he observes: "Since that time I have extended and diversified the experiments, and have observed, that what was done by air and water will be done by any two kinds of air, and whether they have affinity to one another or not; that this takes place in circumstances of which I was not at all apprised before, and such as experimenters ought to be acquainted with, in order to prevent mistakes of considerable consequence."

The facts stated above, taken altogether, appear to me to form as decisive evidence for that theory of elastic fluids which I maintain, and against the one commonly received, as any physical principle which has ever been deemed a subject of dispute can adduce.

III. *On the Absorption of Gases by Water and other Liquids.* By JOHN DALTON*.

1. IF a quantity of pure water be boiled rapidly for a short time in a vessel with a narrow aperture, or if it be subjected to the air-pump, the air exhausted from the receiver containing the water, and then be briskly agitated for some time, very nearly the whole of any gas the water may contain will be extricated from it.

2. If a quantity of water thus freed from air be agitated in any kind of gas not chemically uniting with water, it will absorb its bulk of the gas, or otherwise a part of it equal to some one of the following fractions, namely, $\frac{1}{8}$, $\frac{1}{27}$, $\frac{1}{64}$, $\frac{1}{125}$, &c., these being the cubes of the reciprocals of the natural numbers 1, 2, 3, &c., or $\frac{1}{1^3}$, $\frac{1}{2^3}$, $\frac{1}{3^3}$, $\frac{1}{4^3}$, &c., the same gas always being absorbed in the same proportion, as exhibited in the following table. It must be understood that the quantity of gas is to be measured at the pressure and temperature with which the impregnation is effected.

Bulk absorbed, the bulk of water being unity.	
$\frac{1}{1^3} = 1$	Carbonic acid gas, sulphuretted hydrogen, nitrous oxide†.
$\frac{1}{2^3} = \frac{1}{8}$	Olefiant gas of the Dutch chemists.
$\frac{1}{3^3} = \frac{1}{27}$	Oxygenous gas, nitrous gas‡, carburetted hydrogen gas, from stagnant water.
$\frac{1}{4^3} = \frac{1}{64}$	Azotic gas, hydrogenous gas, carbonic oxide.
$\frac{1}{5^3} = \frac{1}{125}$	None discovered.

* From *Manchester Transactions*, second series, vol. i.

† According to Mr. William Henry's experiments, water does not imbibe quite its bulk of nitrous oxide: in one or two instances with me it has come very near it. The apparent deviation of this gas may be owing to the difficulty of ascertaining the exact degree of its impurity.

‡ About 1-26th of nitrous gas is usually absorbed, and 1-27th is recovera-

3. The

3. The gas thus absorbed may be recovered from the water the same in quantity and quality as it entered, by the means pointed out in the first article.

4. If a quantity of water free from air be agitated with a mixture of two or more gases (such as atmospheric air), the water will absorb portions of each gas the same as if they were presented to it separately in their proper density.

Ex. gr. Atmospheric air, consisting of 79 parts azotic gas, and 21 parts oxygenous gas, per cent.

Water absorbs $\frac{1}{0\cdot4}$ of $\frac{79}{100}$, azotic gas = 1·234
——————— $\frac{1}{2\cdot7}$ of $\frac{21}{100}$, oxygen gas = ·778

Sum, per cent. 2·012

5. If water impregnated with any one gas (as hydrogenous) be agitated with another gas equally absorbable (as azotic), there will apparently be no absorption of the latter gas, just as much gas being found after agitation as was introduced to the water; but upon examination the residuary gas will be found a mixture of the two, and the parts of each, in the water, will be exactly proportional to those out of the water.

6. If water impregnated with any one gas be agitated with another gas less or more absorbable, there will apparently be an increase or diminution of the latter; but upon examination the residuary gas will be found a mixture of the two, and the proportions agreeable to article 4.

7. If a quantity of water in a phial, having a ground stopper very accurately adapted, be agitated with any gas, or mixture of gases, till the due share has entered the water; then, if the stopper be secured, the phial may be exposed to any variation of temperature without disturbing the equilibrium: that is, the quantity of gas in the water will remain the same whether it be exposed to heat or cold, if the stopper be air-tight.

ble: this difference is owing to the residuum of oxygen in the water, each measure of which takes 3½ of nitrous gas to saturate it when in water. Perhaps it may be found that nitrous gas usually contains a small portion of nitrous oxide.

N. B. The

N. B. The phial ought not to be near full of water, and the temperature should be between 32° and 212°.

8. If water be impregnated with one gas (as oxygenous, and another gas, having an affinity for the former (as nitrous), be agitated along with it, the absorption of the latter gas will be greater, by the quantity necessary to saturate the former, than it would have been if the water had been free from gas *.

9. Most liquids free from viscidity, such as acids, alcohol, liquid sulphurets, and saline solutions in water, absorb the same quantity of gases as pure water, except they have an affinity for the gas, such as the sulphurets for oxygen, &c.

The preceding articles contain the principal facts necessary to establish the theory of absorption; those that follow are of a subordinate nature, and partly deducible as corollaries to them.

10. Pure distilled water, rain and spring water, usually contain nearly their due share of atmospheric air; if not, they quickly acquire that share by agitation in it, and lose any other gas they may be impregnated with. It is remarkable, however, that water by stagnation, in certain circumstances, loses part or all of its oxygen, notwithstanding its constant exposition to the atmosphere. This I have uniformly found to be the case in my large wooden pneumatic trough, containing about eight gallons, or $1\frac{1}{3}$ cubic foot of water. Whenever this is replenished with tolerably pure rain water, it contains its share of atmospheric air, but in process of time it becomes deficient of oxygen: in three months the whole surface has been covered with a pellicle, and no oxygenous gas whatever was found in the water. It was grown offensive, but not extremely so; it had not been contaminated with any material portion of

* One part of oxygenous gas requires 3·4 of nitrous gas to saturate it in water. It is agreeable to this that the rapid mixture of oxygenous and nitrous gas over a broad surface of water occasions a greater diminution than otherwise. In fact, the nitrous acid is formed this way; whereas when water is not present the nitric acid is formed, which requires just half the quantity of nitrous gas, as I have lately ascertained.

metallic or sulphurcous mixtures, or any other article to which the effect could be ascribed *. The quantity of azotic gas is not materially diminished by stagnation, if at all. These circumstances, not being duly noticed, have been the source of great diversity in the results of different philosophers upon the quantity and quality of atmospheric air in water. By article 4, it appears that atmospheric air expelled from water ought to have 38 per cent. oxygen; whereas by this article air may be expelled from water that shall contain from 38 to 0 per cent. of oxygen. The disappearance of oxygenous gas in water, I presume, must be owing to some impurities in the water which combine with the oxygen. Pure rain water that had stood more than a year in an earthen-ware bottle had lost none of its oxygen.

11. If water free from air be agitated with a small portion of atmospheric air (as 1-15th of its bulk), the residuum of such air will have proportionally less oxygen than the original: if we take 1-15th, as above, then the residuum will have only 17 per cent. oxygen, agreeably to the principle established in article 4. This circumstance accounts for the observations made by Dr. Priestley and Mr. William Henry, that water absorbs oxygen in preference to azote.

12. If a tall glass vessel, containing a small portion of gas, be inverted into a deep trough of water, and the gas thus confined by the glass and the water be briskly agitated, it will gradually disappear.

It is a wonder that Dr. Priestley, who seems to have been the first to notice this fact, should have made any difficulty of it: the loss of gas has evidently a mechanical cause; the agitation divides the air into an infinite number of minute bubbles, which may be seen pervading the whole water; these are successively driven out from under the margin of the glass into the trough, and so escape.

13. If old stagnant water be in the trough in the last experiment, and atmospheric air be the subject, the oxygenous gas will very soon be almost wholly extracted, and leave a residuum of azotic gas; but if the water be fully

* It was drawn from a leaden cistern.

impregnated

impregnated with atmospheric air at the beginning, the residuary gas, examined at any time, will be pure atmospheric air.

14. If any gas, not containing either azotic or oxygenous gas, be agitated over water containing atmospheric air, the residuum will be found to contain both azotic and oxygenous gas.

15. Let a quantity of water contain equal portions of any two or more unequally absorbable gases; for instance, azotic gas, oxygenous gas, and carbonic acid gas; then let the water be boiled, or subjected to the air pump, and it will be found that unequal portions of the gases will be expelled: the azotic will be the greatest part, the oxygenous next, and the carbonic acid will be the least. For, the previous impregnation being such as is due to atmospheres of the following relative forces nearly,

Azotic - -	21 inches of mercury
Oxygenous -	9 ditto
Carbonic acid	0½ ditto

consequently, when those forces are removed, the resiliency of the azotic gas will be the greatest, and that of the carbonic acid the least; the last will even be so small as not to overcome the cohesion of the water without violent agitation.

Remarks on the Authority of the preceding Facts.

In order to give the chain of facts as distinct as possible, I have not mentioned by whom, or in what manner, they were ascertained.

The fact mentioned in the first article has been long known; a doubt, however, remained respecting the quantity of air still left in water after ebullition and the operation of the air pump. The subsequent articles will, I apprehend, have placed this in a clearer point of view.

In determining the quantity of gases absorbed, I had the result of Mr. William Henry's experience on the subject before me, an account of which has been published in the Philosophical Transactions for 1803. By the reciprocal communications since, we have been enabled to bring the

results of our experiments to a near agreement, as the quantities he has given in his appendix to that paper nearly accord with those I have stated in the second article. In my experiments with the less absorbable gases, or those of the 2d, 3d, and 4th classes, I used a phial holding 2700 grains of water, having a very accurately ground stopper; in those with the more absorbable of the first class I used an eudiometer tube properly graduated, and of aperture so as to be covered with the end of a finger. This was filled with the gas, and a small portion expelled by introducing a solid body under water: the quantity being noticed by the quantity of water that entered on withdrawing the solid body, the finger was applied to the end, and the water within agitated: then, removing the finger for a moment under water, an additional quantity of water entered, and the agitation was repeated till no more water would enter, when the quantity and quality of the residuary gas were examined. In fact, water could never be made to take its bulk of any gas by this procedure; but if it took 9-10ths, or any other part, and the residuary gas was 9-10ths pure, then it was inferred that water would take its bulk of that gas. The principle was the same in using the phial; only a small quantity of the gas was admitted, and the agitation was longer.

There are two very important facts contained in the second article. The first is, that the quantity of gas absorbed is as the density or pressure. This was discovered by Mr. William Henry, before either he or I had formed any theory on the subject.

The other is, that the density of the gas in the water has a special relation to that out of the water, the distance of the particles within being always some multiple of that without. Thus, in the case of carbonic acid, &c. the distance within and without is the same, or the gas within the water is of the same density as without; in olefiant gas the distance of the particles in the water is twice that without; in oxygenous gas, &c. the distance is just three times as great within as without; and in azotic, &c. it is four times. This fact was the result of my own inquiry. The

former

former of these, I think, decides the effect to be mechanical; and the latter seems to point to the principle on which the equilibrium is adjusted.

The facts noticed in the 4th, 5th, and 6th articles were investigated, *à priori*, from the mechanical hypothesis, and the notion of the distinct agency of elastic fluids when mixed together. The results were found entirely to agree with both, or as nearly as could be expected from experiments of such nature.

The facts mentioned in the 7th article are of great importance in a theoretic view; for, if the quantity of gas absorbed depend upon mechanical principles, it cannot be affected by temperature in confined air, as the mechanical effect of the external and internal air is alike increased by heat, and the density not at all affected in those circumstances. I have tried the experiments in a considerable variety of temperature without perceiving any deviation from the principle. It deserves further attention.

If water be, as pointed out by this essay, a mere receptacle of gases, it cannot affect their affinities: hence what is observed in the 8th article is too obvious to need explanation. And if we find the absorption of gases to arise not from a chemical but a mechanical cause, it may be expected that all liquids having an equal fluidity with water will absorb like portions of gas. In several liquids I have tried, no perceptible difference has been found; but this deserves further investigation.

After what has been observed, it seems unnecessary to add any explanation of the 10th and following articles.

Theory of the Absorption of Gases by Water, &c.

From the facts developed in the preceding articles, the following theory of the absorption of gases by water seems deducible:

1. All gases that enter into water and other liquids by means of pressure, and are wholly disengaged again by the removal of that pressure, are mechanically mixed with the liquid, and not chemically combined with it.

2. Gases so mixed with water, &c. retain their elasticity

or repulsive power amongst their own particles, just the same in the water as out of it, the intervening water having no other influence in this respect than a mere vacuum.

3. Each gas is retained in water by the pressure of gas of its own kind incumbent on its surface abstractedly considered, no other gas with which it may be mixed having any permanent influence in this respect.

4. When water has absorbed its bulk of carbonic acid gas, &c. the gas does not press on the water at all, but presses on the containing vessel just as if no water were in. When water has absorbed its proper quantity of oxygenous gas, &c. that is, $\frac{1}{27}$ of its bulk, the exterior gas presses on the surface of the water with $\frac{26}{27}$ of its force, and on the internal gas with $\frac{1}{27}$ of its force, which force presses upon the containing vessel and not on the water. With azotic and hydrogenous gas the proportions are $\frac{63}{64}$ and $\frac{1}{64}$ respectively. When water contains no gas, its surface must support the whole pressure of any gas admitted to it, till the gas has, in part, forced its way into the water.

5. A particle of gas pressing on the surface of water is analogous to a single shot pressing upon the summit of a square pile of them. As the shot distributes its pressure equally amongst all the individuals forming the lowest stratum of the pile, so the particle of gas distributes its pressure equally amongst every successive horizontal stratum of particles of water downwards till it reaches the sphere of influence of another particle of gas. For instance, let any gas press with a given force on the surface of water, and let the distance of the particles of gas from each other be to those of water as 10 to 1; then each particle of gas must divide its force equally amongst 100 particles of water, as follows:— It exerts its immediate force upon 4 particles of water; those 4 press upon 9, the 9 upon 16, and so on according to the order of square numbers, till 100 particles of water have the force distributed amongst them; and in the same stratum each square of 100, having its incumbent particle of gas, the water below this stratum is uniformly pressed by the gas, and consequently has not its equilibrium disturbed by that pressure.

When

When water has absorbed $\frac{1}{27}$ of its bulk of any gas, the stratum of gas on the surface of the water presses with $\frac{26}{27}$ of its force on the water, in the manner pointed out in the last article, and with $\frac{1}{27}$ of its force on the uppermost stratum of gas in the water: the distance of the two strata of gas must be nearly 27 times the distance of the particles in the incumbent atmosphere, and 9 times the distance of the particles in the water. This comparatively great distance of the inner and outer atmosphere arises from the great repulsive power of the latter, on account of its superior density, or its presenting 9 particles of surface to the other 1. When $\frac{1}{64}$ is absorbed, the distance of the atmospheres becomes 64 times the distance of two particles in the outer, or 16 times that of the inner. The annexed views of perpendicular and horizontal strata of gas in and out of water, will sufficiently illustrate these positions.

7. An equilibrium between the outer and inner atmospheres can be established in no other circumstance than that of the distance of the particles of one atmosphere being the same or some multiple of that of the other; and it is probable the multiple cannot be more than 4. For in this case the distance of the inner and outer atmospheres is such as to make the perpendicular force of each particle of the former on those particles of the latter that are immediately subject to its influence, physically speaking, equal; and the same may be observed of the small lateral force.

8. The greatest difficulty attending the mechanical hypothesis, arises from different gases observing different laws. Why does water not admit its bulk of every kind of gas alike?—This question I have duly considered: and though I am not yet able to satisfy myself completely, I am nearly persuaded that the circumstance depends upon the weight and number of the ultimate particles of the several gases; those whose particles are lightest and single being least absorbable, and the others more, according as they increase in weight and complexity*. An inquiry into the relative weights of the ultimate particles of bodies, is a subject, as

* Subsequent experience renders this conjecture less probable.

far as I know, entirely new: I have lately been prosecuting this inquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results, as far as they appear to be ascertained by my experiments.

Table of the relative Weights of the ultimate Particles of Gaseous and other Bodies.

Hydrogen	1
Azote	4·2
Carbon	4·3
Ammonia	5·2
Oxygen	5·5
Water	6·5
Phosphorus	7·2
Phosphuretted hydrogen	8·2
Nitrous gas	9·3
Ether	9·6
Gaseous oxide of carbon	9·8
Nitrous oxide	13·7
Sulphur	14·4
Nitric acid	15·2
Sulphuretted hydrogen	15·4
Carbonic acid	15·3
Alcohol	15·1
Sulphureous acid	19·9
Sulphuric acid	25·4
Carburetted hydrogen from stagnant water	6·3
Olefiant gas	5·3

In Plate I. fig. 1. the lower globes are to represent particles of water: the top globe represents a particle of air resting on four particles of water. Fig. 3. Incumbent stratum of atmosphere—the particles marked with a dot or point. A stratum of air in the water—the particles marked with a small cross. Two representations are given: in one (azotic and hydrogenous gas), the distance of particles 4 to 1; in the other (oxygenous, nitrous, and carburetted hydrogenous gas), distance of particles 3 to 1.

Plate II. fig. 1. Profile view of air in water, azotic and hydrogenous gas, $\frac{1}{6\frac{1}{4}}$ density: oxygenous, nitrous, and carburetted hydrogenous gas, $\frac{1}{9\frac{1}{7}}$ density.

IV. *An*

IV. *An Experimental Inquiry into the Nature of Gravelly and Calculous Concretions in the Human Subject; and the Effects of Alkaline and Acid Substances on them, in and out of the Body.* *By* THOMAS EGAN, *M.D. M.R.I.A.*

[Continued from our last volume, p. 307.]

PART II.

THE bad effects of all acid and acescent substances being generally felt and acknowledged, we cannot be surprised that sufferers from these maladies should naturally expect an alleviation of their complaints from substances of a very opposite nature; or that, perhaps, in the general anxiety of mankind to discover a solvent of these concretions, the active agency of alkaline matters could not be overlooked. We accordingly find that, from the remotest antiquity up to this day, they were, and still are, though under various modifications, chiefly resorted to. Our antient physicians prescribed waters with mineral alkaline impregnation, such as Seltzer, Carlsbad, and others; and, in latter times, we find our own countrymen more particularly engaged in these pursuits. Lime water, recommended by White, (to whose numerous and interesting experiments I must beg leave to refer;) lime, and pure alkaline matter, forming the bases of the celebrated remedies of madam Stephens, Hartley, and others. And, in our own days, the caustic lixivium, again forgot, to make room for the more modern and fashionable introduction of both our alkaline sub- and super-carbonates; the vegetable, as in Faulknor's mephitic alkaline water, or in the crystallized carbonate of potash; the mineral, in a desiccated state, as recommended by my learned and indefatigable friend, Dr. Beddoes; or in that of the well known soda waters, first introduced in Geneva and Paris.

Now, in whatever of the above forms these saline matters are employed, their decided good effects are universally experienced and acknowledged. The *aqua mephitica alkalina* I consider the most valuable gift bestowed upon mankind

kind by our modern chemistry; and to Beddoes's desiccated soda pills, my colleagues must join with me in acknowledging our greatest obligations. But how account for these good effects? or what can their *modus operandi* be? *Hic labor, hoc opus.* Carbonates, we have always been given to understand, exerted no solvent power on gravelly or calculous matter; and this continues to be the opinion of philosophers, as well as medical chemists, to this day. We find Fourcroy, in his late elaborate work on this subject, still continuing to assert, (in mentioning the action of various matters upon uric acid,) "les carbonates alkalines n'ont aucune action sur lui." Nor does the difficulty diminish with respect to the pure alkalies; for, in the stomach and *primæ viæ*, they must return again, to either a carbonated or saponaceous state. My ingenious friend, and master in chemistry, Mr. William Higgins, (in the work already quoted,) emphatically exclaims, "Why not at once give soap? why not turn our attention to the mild mineral alkali?" With regard to the common alkaline carbonates in use, it may be observed that the saturation is not complete; and that the uncombined portion of alkaline matter may still exert its specific powers observable by its detergent quality, as has been so long since well explained by Mr. Kirwan. This explanation, however, could not extend to the *potassa carbonata*, or crystallized vegetable alkali, lately introduced, and with equal success. May I be permitted (notwithstanding its use as a test, and non-deliquescence,) to entertain some doubt of its complete saturation? for that, prepared with the most scrupulous care, still retains its alkaline taste, and acts with energy on the vegetable blues. The carbonic may probably be too weak an acid to entirely annul its alkaline property in any proportion that we can possibly unite them in the solid state. I am informed by Mr. Kirwan, however, that this can be effected; but the saturation is temporary, and continues only during its most recent state. This we now accomplish, by mechanical means, in the fluid one of our soda and other mineral waters; where, indeed, the alkali may be considered as in the super-carbonated state. Now, the success attendant

attendant also upon their exhibition, completely does away my former hypothesis; and we are left to conclude, that either the opinion of their want of action upon gravelly and calculous matter is unfounded, or that the animal œconomy may be possessed (among the multiplicity of its wonders) of some unknown chemical agency, whereby it may, in their course through the circulation, disengage their carbonic acid gas.

This would not appear more extraordinary than the formation of the different and most opposite secretions, such as bile and milk, from one and the same fluid; nor than what we every day observe to pass in the functions of the vegetable department of the organized kingdom. The *salsola kali, salicornia,* and other maritime plants, afforded to Chaptal's analysis, in their early state of growth, muriate of soda; when the plant was more advanced, this salt, with this excess of alkali; but in a full state of maturity, the same quite disengaged, and uncombined with muriatic acid. Here, then, we have one of our most refractory salts, and that resists the action of our greatest fires, completely decomposed by the vegetative powers of an humble plant. In this state of uncertainty I determined upon a course of some experiments which might throw some light on this subject; and go to explain how, or upon what principle, alkaline earths or carbonates become remedies in those complaints. And here, again, I must bring to our recollection, that whatever retains the uric acid in a state of solution whilst in the body, must prevent the formation of gravel or calculous matter of that kind.

And to begin with lime water, so generally prescribed since the time of White. In this, the quantity of pure earth in solution (being only one grain in 700 of water) is so minute, and it is, withal, so readily decomposed, that we could not, *à priori*, expect much from its agency.

On conversing, however, with my friend Dr. Harvey on this subject, (of whose professional acumen it is unnecessary to make mention here,) he observed, with that strength of reasoning peculiarly his own: "Whatever may be the result of your future inquiries, can you, for a moment, imagine

gine that physicians of the first eminence, and of all nations, would still consent to tread in the path of empiricism by persevering in the use of this remedy, if they were not retained in it by the irresistible evidence of a successful practice and observation? or that the late Dr. Smyth, a gentleman of great discernment, and extensive knowledge, would, so generally and promiscuously, prescribe lime water in gout and gravel, if he were not satisfied of its efficacy, as well as of the great similarity of these complaints?"

Experiment I.

To four ounces of healthy urine was added one ounce of lime water. A similar quantity of urine was set aside as a standard, both in close vessels; temperature varying from 60 to 75 degrees, being in August 1799. In the first, no sign of the slightest separation, or crystallization of uric acid, after three, five, or seven days. Some observable in the standard after the third day, which increased in quantity to the fifth.

Experiment II.

To the same quantity of urine was added half an ounce only of lime water, with the same appearances as before. No sign of uric acid after several days. The standard deposited a few crystals on the third morning.

Experiment III.

To the same quantity of urine were added two drachms only of lime water, which, though insufficient to neutralize the disengaged phosphoric acid, yet seemed as effectually to prevent the separation of uric acid as the greater quantities employed in the former experiments.

Experiment IV.

To three ounces of the urine of a child, six years old, still warm, and subject to deposit gravel upon cooling, were added two drachms of lime water, which effectually prevented all separation of this matter, whilst the standard precipitated copiously this saline substance after three hours.

Experiment

Experiment V.

To three ounces of the same kind of urine as in the preceding, was added one drachm of lime water: the result the same as in the former. Some precipitation in the standard, as in the preceding experiment.

When we consider the small proportion of lime kept in solution in water, and that the lime water used in my experiments was far from being recent, we must be astonished at the minute quantity that proves sufficient to keep the uric acid in solution: but this wonder ceases when we recollect that the proportion of it in the above quantities of urine is extremely small, and that it is scarcely acid; as we may learn from the controversy that took place between two such able chemists as Pearson and Fourcroy on this subject. Finding, then, our common lime water exerting such powers in preventing the separation or crystallization of this substance, it occurred to me, that much more might be expected from barytic lime water, as containing a larger proportion of saline matter in solution; and that, though, from its poisonous effects in the carbonated state, the internal exhibition would be hazardous, yet it might prove an useful remedy when injected into the bladder. But how uncertain are our apparently best founded theories when not deduced from experiment!

Experiment VI.

To three ounces of urine was added one drachm of barytic lime water, which immediately seemed to decompose the whole, render it turbid, and give it the appearance of the *urina jumentosa*; for reasons easily and satisfactorily explained by Fourcroy, to whom I must refer. After two days I was surprised to find some small crystalline matter adhering to the sides of the glass; and, upon examining the copious precipitate from this re-agent, I found it blended with as much crystallized uric acid as appeared in the standard. A repetition of this experiment, both since and at that time, afforded the same results. Now the strength of barytic lime water is, to that of the common, nearly as 13 to 1; the former keeping in solution, at the temperature 60

degrees, 13 grains to the ounce. Has the barytic, with all its superior energy, less affinity for the uric acid than the calcareous earth? or does a superior affinity, to some other ingredients of the urinary compound, supersede its union to this?

I regret that the small quantity of stronthian lime water, which I possessed, did not permit me to extend my inquiries with it.

Finding, then, that our alkaline earth of lime, in the weakest possible state of solution, and in the smallest proportion, effectually prevents the crystallization, or keeps in solution the lithic acid of urine: if we only suppose that it reaches the kidneys and bladder in the smallest quantity, it must produce similar effects there, obviate the further formation of gravelly matter, or further accumulation of pre-existing calculous concretions of this kind.

Let us now proceed to inquire into the effects of the pure and carbonated alkalies themselves. The action of the former being well known and acknowledged, I shall content myself with one experiment, and pass on to the latter.

Experiment VII.

To four ounces of the urine of a child often depositing gravel, on cooling, were added ten drops of the *aqua kali puri* of the shops. After seven days no sign of separation: some observable in the standard after some hours.

Experiment VIII.

To four ounces of urine were added three grains only of crystallized carbonate of potash, the purity of which was ascertained by Dr. Perceval and myself, and containing, according to Mr. Kirwan, 1·23 grains of alkali. After several days no appearance of crystallized matter: some in the standard after forty-eight hours.

Experiment IX.

To the same quantity of urine were added two grains only of the same, with the like result.

From the pure and carbonated, let us now proceed to the super-carbonated and sub-carbonated states.

Experiment X.

To three ounces of urine, in a well closed phial, was added half an ounce of the *aqua mephitica alkalina*, prepared according to Dr. Faulkner's proportions. No crystallized appearance after seven days. This result we might well expect, from the relative large proportion of its alkaline salt; having already seen equally good effects, from half an ounce only of Kinsley's soda water, containing a mere fraction of alkaline matter.

Experiment XI.

To three ounces of urine were added two grains of the common salt of tartar of the shops, containing, according to Mr. Kirwan, 0·82 of alkali. The same results as in the former experiments, even after six days.

Experiment XII.

Having no pure mineral alkali, three grains of the common crystallized soda of the shops, containing, according to Mr. Kirwan, 0·64 of alkali, were added to four ounces of urine. The result as in the former; which was equally produced by two grains only; and perhaps would have been equally so by one.

Wishing to be more fully convinced that the very large proportion of disengaged carbonic acid gas in our soda waters did not counteract the usual alkaline effects:

Experiment XIII.

To four ounces of the urine of the same child, which was generally surcharged with gravelly matter, I added half an ounce of Kinsley's soda water in its full state of effervescence: the phial well corked, and removed into a cold cellar, temperature 42 degrees. After four days, nay, a week, nothing but the usual calcareous sediment, without an atom of crystallized, or otherwise precipitated uric acid.

From the above experiments, then, we learn, that pure lime in the state of lime water, the pure alkalies, the subcarbonated, carbonated, and super-carbonated, all prevent the separation of the uric acid, by uniting probably with,

and

and retaining it in solution. That they should still exert their power in the super-carbonated soda water, is rather singular; and we must suppose that, in the temperature of the human body, this superabundant gas (which, for the greater part, is only retained by compression) would be disengaged, and leave the alkali to exert its usual properties; and so, I would presume, it happens.

A half pint of soda water was poured into a large glass, and exposed to the influence of the atmosphere in a temperature of from 60 to 75 degrees. After two days it continued to turn litmus red, and only ceased to do so at the end of three. But in Experiment X. we find it in its full gaseous state, still possessing its alkaline influence on the uric acid; which I would be disposed to attribute to its very weak union to the carbonic acid in the fully carbonated and super-carbonated states, as well as to the very weak degree of acidity of the uric acid itself, rendering the most minute portion of all alkaline matter sufficient to its saturation. However this may be, it is obvious that the extraordinary quantity of gas with which these waters are surcharged, is undoubtedly superfluous, and may probably prove dangerous. In gouty habits (so subject to these complaints) there is always danger of their inducing spasmodic affections of the stomach. This has frequently occurred; and if, to prevent it, we are obliged to add spirituous tinctures, and brandy, why not as well omit this super-saturation at once, and content ourselves with that pleasing degree of it which exceeds but little that of saturation?

Nor have the predisposed to apoplexy less to apprehend. And in these cases we find our own physicians, as well as those of the sister kingdom, preferring carbonated potash, or desiccated soda. But, recollecting that I am acting, on this occasion, the part only of the experimenter, I shall now proceed to consider what the action of these saline substances may be on the uric acid, in its concrete or calculous state, as well as on a few others of these concretions, which, though of a different nature, are of frequent occurrence, and easy solubility. The nature of those employed in the following

experiments

experiments was always previously ascertained and specified; they were also carefully weighed and dried, both before and after immersion in their several menstrua.

Dr. Percival, of Manchester, as well as others, having experienced the solvent power of the plain mephitic, or carbonated water, on urinary calculi, it was thought proper to repeat his experiments.

Experiment I.

A fragment of a calculus, weighing twenty-three grains, and of the uric acid kind, was suspended by a thread, for forty-eight hours, in Nooth's apparatus, already nearly filled with highly impregnated aërated water, and still exposed to a stream of carbonic acid gas: temperature 58 degrees. When taken out, and dried, weighed, as before, twenty-three grains.

Experiment II.

A fragment of a calculus, of the same kind, weighing forty grains and three quarters, was suspended, as before, in Nooth's apparatus, for forty-eight hours: temperature varying from forty to fifty-five degrees. On being taken out, and dried, was found to have sustained no loss.

Experiment III.

An entire calculus, of a rough and sandy appearance, chiefly of the uric acid kind, but with some extremely minute intermixed particles of the ammoniacal magnesian phosphate, weighing fifty-two grains, was suspended, as before, for forty-eight hours, in Nooth's apparatus. After being taken out, and dried, was found to weigh fifty-one grains and a quarter; so that there was here a loss of three quarters of a grain: undoubtedly of the ammoniacal magnesian phosphate.

Experiment IV.

Wishing to see whether even increased temperature would add to the solvent power of carbonic acid, a fragment of calculus, of the uric acid kind, weighing twenty-two grains, was immersed, as before, for forty-eight hours, in three ounces and a half of highly impregnated carbonated water,

in a well closed phial, and laid on a sand heat which did not exceed the temperature of 100 degrees. After being taken out, and dried, the weight was found, as before, twenty-two grains.

From these experiments, then, we may conclude, that calculi of the uric acid kind are insoluble in carbonated water; and that Dr. Percival, whose character as a philosopher, as well as a physician, deservedly stands so high, must have operated upon concretions of a different kind; more especially as in his experiments there was a loss of several grains in only a few ounces of mephitic water, whilst none appeared in ours, though in several pounds of that fluid. He must have then operated upon some of a different and highly soluble kind.

Experiment V.

One-half of a calculus, of the ammoniacal magnesian kind, weighing 100·5 grains, was suspended, as usual, for forty-eight hours, in Nooth's apparatus: temperature 50 degrees. Upon being taken out, and dried, was found to weigh 92·63 grains; so that its loss amounted to 7·87, or rather more than seven grains three quarters. And here we have an explanation of the result of Dr. Percival's experiments, by supposing that the calculi he employed must have been of this species; the extreme solubility of which, in so weak and innocent a menstruum, should excite the earnest hope of our young gentlemen, in the surgical department, of effecting its solution by injections of carbonated water into the bladder. To such safe trials they must be encouraged by the pleasing consideration that this kind forms a large proportion of the human urinary calculi. On these occasions the water should not be too highly impregnated, lest the sudden expansion of the gas under the human temperature might excite the bladder to reject it too quickly. This inconvenience, however, must be in part obviated by the necessity of previously warming the injection to about the temperature of 80 degrees; a precaution never to be omitted. But to return to the alkaline earths and salts:—

Experiment VI.

One-half of a uric acid calculus was suspended, for forty-eight

eight hours, in four ounces of lime water: temperature as before. After being dried, was found to have lost seven grains three quarters; and the surface to be covered with a granular efflorescence, which, in drying, detaches itself. The calculus was so much softened as to leave little doubt of its entire destruction by a few more immersions. It was again suspended for a month, in the same quantity of lime water, in the temperature of the atmosphere only, without any renewal of the menstruum; when it was found to have lost twenty-four grains. Now, if the lime water had been frequently renewed, and its energy assisted by the standard heat of the human body, no doubt but it would have been entirely broken down in a much shorter time. We find, then, lime water not only preventing the separation of uric acid from urine, but acting powerfully upon it in its most compact form. How well founded, then, were the experiments of Whyte, as well as the opinion of Dr. Smyth; and how little deserving the latter the obloquy of his contemporaries for his predilection to it! Now this result, corresponding also with Scheele's, points it out to us as one of our most safe and active agents, when injected into the bladder with the necessary precautions. And we must feel surprised that no attempts of that kind have been made since the time of Whyte.

Experiment VII.

A fragment of calculus, of the uric acid kind, weighing seventy-nine grains and a quarter, was suspended, for forty-eight hours, in a mixture consisting of four ounces of distilled water and twenty-five drops of the weak *aqua kali puri* of the shops, which merely gave it an alkaline taste. It was then placed on a sand heat; temperature varying occasionally from 60 to 100 degrees. After being taken out, and dried, it weighed seventy-four grains and three quarters; so that the loss amounted to four grains and a half. This weak lixivium, it appears, then, operated upon it, acquired a yellow colour, a sweet taste, and precipitated, with a few drops of muriatic acid, a white sediment, easily recognizable

by the small, silky, needle-shaped, crystalline appearance peculiar to the uric acid.

Experiment VIII.

This same fragment of calculus, after being well washed, and dried, was again immersed, for forty-eight hours, in a lixivium consisting of only twenty drops of *aqua kali puri* to four ounces of distilled water, and under the same circumstances. Upon being taken out, and dried, it was found to have lost four grains and a quarter. The solution was of a yellowish green colour, lost all alkaline taste, and precipitated, as before, with either the acetous or muriatic acid.

Experiment IX.

A fragment of calculus, of the same kind, weighing eighty-one grains, was suspended, as before, for forty-eight hours, in a mixture consisting only of fifteen drops of the same alkali to four ounces of distilled water, which scarcely imparted an alkaline taste. After being taken out, and dried, it was found to have lost one grain and three quarters; the specimen consisting chiefly of the external laminæ, much more slowly acted upon.

Experiment X.

This same fragment, washed and dried, was again immersed, for forty-eight hours, in a similar lixivium, and under the like circumstances. The loss now amounted to nearly four grains; and from this we learn how considerably the energy of the menstruum is increased by each succeeding immersion; so much so, indeed, that a few repetitions enable it to disunite the laminæ, and cause them to crumble into a pulverulent state, easily voidable with the urine. To the happy result, therefore, of this experiment let me earnestly solicit the attention of our young practitioners.

[To be continued.]

V. *The*

V. *The Syphon applied to the Worm Tub as a Refrigerator; or, A Plan for conveying Water in any Quantity to a Worm Tub of the largest Dimensions, if perfectly Air tight. By* ALEXANDER JOHNSTON, *Engineer**.

A. FIG. 2. Plate II. The feed pipe of cold water.

B. The hot water, or waste pipe, the end of which must be about two feet lower than the feed pipe, to make it act with full effect.

When you commence work, you must shut the cocks, and fill the tub through a hole at top, and, of course, both pipes, and, when full, stop the hole at top, and open the cocks together; the water will then commence running, and continue as long as the supply holds good, as it acts in every respect on the principle of a syphon.

By this means pumps, horse mills, and other machinery, are rendered unnecessary for that purpose.

The application of this improvement is simple, and executed at a very little expense. The saving, I think, may be calculated at upwards of one hundred horses per annum, for the city of Dublin alone.

I have executed one for Nicholas Roe, esq. Marybone-lane.

ALEXANDER JOHNSTON.

Dublin, July 6, 1803.

VI. *Description of a Cometarium invented by* EZ. WALKER, *Esq.*

To Mr. Tilloch.

SIR, Lynn, Jan. 21, 1806.

I HAVE lately constructed a cometarium, which produces a more beautiful effect than any instrument of the kind that I am acquainted with. It is founded on a property of the convex lens, that I do not remember to have seen taken notice of by any writer on optics; and thinking that it may

* From *Transactions of the Dublin Society*, vol. iii.

afford amusement to some of your readers, I have taken the liberty to trouble you with a description of it.

The first part of my instrument that I shall describe consists of a mahogany board, 14 inches in length and 10 in breadth, fixed to a stand in a perpendicular direction, in the following manner:

A brass pin, sufficiently strong to bear the weight of the instrument, is screwed into the side of a stand of a telescope; the other end of the pin is tapped, and has a button screw that turns upon it. This pin goes through a hole near the bottom of the board, and by means of the button screw and brass washers the board is made fast to the stand. Near the top of the board a hole 6 inches wide and 3 inches high is cut through it; the top of the hole being 3 inches from the top of the board.

2dly. A double convex lens, 4 inches in diameter and 24 inches focal distance, set in a piece of mahogany 6 inches square, is mounted in a square frame 6 inches within. The lens turns round within this frame upon two pins, like a looking-glass that is made to stand upon a table. This frame is made fast to the board with screws around the hole above mentioned, in such a manner that one part of the lens may be on one side of the board, and the other part on the other side. That side of the board on which the frame is fixed, I shall, for the sake of distinction, call the back part of the instrument.

About 2¼ inches below the centre of this lens, another lens of half an inch in diameter, and 36 inches focal distance, is placed on the back part of the instrument, against a hole made through the mahogany board. As this lens requires adjustment, it is set in a small piece of wood that slides in a dove-tail frame fixed to the back part of the instrument; and a lens of 1½ inch diameter, and 40 inches focal distance, is fixed in a hole near the bottom of the board.

Directions for using the Instrument.

Having set the instrument upon a table, with its back turned towards a white wall, at the distance of 23 inches, turn that part of the great lens that is behind the instrument until

until it makes an angle of about 50° with the top part of the frame in which it turns. Then place a lighted candle before the cometarium, at the distance of 18 feet, and the light which passes through the great lens forms a beautiful representation of the tail of a comet upon the wall; the light which passes through the small lens represents its head, and that light which falls upon the other lens is converged to a circular spot, which represents the sun.

When the instrument is turned round by hand on the pin which supports it, the comet revolves round the sun, the head and tail of the comet always keeping in the same position, with respect to the sun, in every part of its orbit.

And as no light is suffered to pass through the instrument except that which goes through the glasses, and no light being in the room except one candle, the luminous pictures are seen, upon the dark shadow made by the opaque parts of the instrument, exceedingly clear and well defined. The head and that part of the tail which joins to it are very brilliant; but this brilliancy of the tail gradually decreases towards its end, as represented in Plate III.

I am, sir, your humble servant,

Ez. Walker.

VII. *A Description of a Property of Caoutchouc, or Indian Rubber; with some Reflections on the Cause of the Elasticity of this Substance. In a Letter to Dr. Holme* *.

Middleshaw, near Kendal,
Nov. 16, 1802.

SIR,

The substance called caoutchouc, or Indian rubber, possesses a singular property, which, I believe, has never been taken notice of in print, at least by any English writer: the present letter contains my experiments and reflections on the subject; and should they appear to deserve the attention of your philosophical friends, I am certain you will take the trouble of communicating the paper to the Literary and Philosophical Society of Manchester.

* From *Manchester Transactions*, second series, vol. i.

The property I am about to describe depends on the temperature of the caoutchouc which is used in the experiment; for heat increases the pliancy of the substance, and cold, on the contrary, renders it more rigid; so that when a slip of this resin has been sufficiently warmed, it may be extended to more than twice its natural length, by a moderate force applied to its extremities; after which it will recover its original dimensions in a moment, provided one of the ends of it be let go as soon as it has been stretched. This disposition of the substance may be produced by a degree of temperature less than the heat of the blood; it is therefore necessary to prepare a slip of it, by steeping it for a few minutes in warm water, or by holding it somewhat longer in the fist: either of these precautions makes the resin pliant, and fits it for the experiment, which is performed in the following manner:

I made a piece of caoutchouc a little heavier than an equal bulk of water, the temperature of which was 45 degrees; the vessel containing the resin and water was then placed on the fire; and when the contents of it were heated to 130 degrees, the caoutchouc floated on the surface.

Experiment I.

Hold one end of the slip, thus prepared, between the thumb and fore-finger of each hand, bring the middle of the piece into slight contact with the edges of the lips, taking care to keep it straight at the time, but not to stretch it much beyond its natural length: after taking these preparatory steps, extend the slip suddenly, and you will immediately perceive a sensation of warmth in that part of the mouth which touches it, arising from an augmentation of temperature in the caoutchouc; for this resin evidently grows warmer the further it is extended, and the edges of the lips possess a high degree of sensibility, which enables them to discover these changes with greater facility than other parts of the body. The increase of temperature, which is perceived upon extending a piece of caoutchouc, may be destroyed in an instant, by permitting the slip to contract again; which it will do quickly by virtue of its own spring, as oft as the stretching force ceases to act as

soon

soon as it has been fully exerted. Perhaps it will be said, that the preceding experiment is conducted in a negligent manner; that a person who wishes for accuracy will not trust his own sense of feeling in inquiries of this description, but will contrive to employ a thermometer in the business. Should the objection be started, the answer to it is obvious; for the experiment in its present state demonstrates the reality of a singular fact, by convincing that sense, which is the only direct judge in the case, that the temperature of a piece of caoutchouc may be changed by compelling it to change its dimensions. The use of a thermometer determines the relative magnitudes of these variations, by referring the question of temperature to the eye: experiments of this sort are therefore of a mathematical nature, and afford a kind of knowledge with which we have nothing to do at present; for we are not inquiring after proportions, but endeavouring to establish the certainty of a fact, which may assist in discovering the reason of the uncommon elasticity observable in caoutchouc. My essay or letter appears to be running into a long digression; the subject must therefore be resumed, and it will not be improper to premise the following simple experiment, in the present state of the inquiry, because it seems capable of affording no inconsiderable degree of insight into the plan which nature pursues in producing the phænomenon in question.

Experiment II.

If one end of a slip of caoutchouc be fastened to a rod of metal or wood, and a weight be fixed to the other extremity, in order to keep it in a vertical position, the thong will be found to become shorter with heat and longer with cold. The processes of heating, cooling, and measuring bodies are so well known, that I need not enter into the minuter parts of the experiment; it will be proper however to add, that an increase of temperature diminishes the specific gravity of the Indian rubber, and a loss of heat occasions a contrary effect in it, as I have proved experimentally. The knowledge of the latter fact leads me to conclude, apparently on reasonable grounds, that the pores or interstices of caoutchouc

chouc are enlarged by heat and diminished by cold; consequently when a slip of this substance which remains extended by a weight, or the application of force, happens to contract from an accession of temperature, the capacity of its pores, taken separately or collectively, is augmented by the change that takes place in the figure of the thong. Now if the existence of caloric be admitted, it will follow from the preceding arguments, that the phænomenon under consideration is occasioned by the alternate absorption and emission of the calorific fluid, in the same manner that ropes, the blades of fuci, as well as many more bodies, are obliged to contract and extend themselves, by the alternate absorption and emission of water. You will perceive by the tenour of the foregoing observations, that my theory of this case of elasticity is perfectly mechanical; in fact, the explanation of it depends upon the mutual attraction of caloric and caoutchouc; the former of which penetrates the latter, and pervades every part of it with the greatest ease and expedition, by which the resin is compelled to accommodate its pores to that portion of the calorific fluid which is due to its whole mass, at any particular degree of temperature. In order to apply the last remark to the phænomenon under consideration, I may observe, that if a force be exerted on a piece of caoutchouc to alter the dimensions of its pores, the mutual attraction mentioned above will resist the effort. But the ease with which this substance may be made to change its figure, and the retractile power which it possesses on these occasions, show that its constituent particles move freely amongst themselves: but where there is motion there is void space; consequently caoutchouc abounds with innumerable pores or interstices, the magnitudes of which are variable, because the specific gravity of the resin becomes less with heat and greater with cold. Now if the dimensions of the pores in a piece of caoutchouc can be lessened, without taking away part of the matter of heat which it contains at the time, this new arrangement in the internal structure of the slip will lessen its capacity for the matter of heat, and consequently augment its temperature. But the warmth of such a slip is increased by stretching it, according

to the first experiment: the pores of it are therefore diminished; and the effort which it exerts at the time arises from the mutual attraction of the caoutchouc and caloric, which attraction causes an endeavour to enlarge the interstices of the former for the reception of the latter: hence it happens that the thong contracts longitudinally, according to the second experiment, and the redundant caloric is absorbed in the course of this operation, which again reduces the temperature. The preceding explanation agrees very well with the phænomenon, as it is stated in the beginning of this letter; and the theory receives additional confirmation from the following facts.

Experiment III.

If a thong of caoutchouc be stretched, in water warmer than itself, it retains its elasticity unimpaired; on the contrary, if the experiment be made in water colder than itself, it loses part of its retractile power, being unable to recover its former figure; but let the thong be placed in hot water, while it remains extended for want of spring, and the heat will immediately make it contract briskly. The foregoing circumstances may be considered as proving, that the elasticity of caoutchouc is not a constitutional quality of the substance, but a contingent effect, arising from the loss of equilibrium between the portion of caloric, which the resin happens to contain at any moment, and its capacity to receive that fluid at the same instant. The object of the present letter is to demonstrate, that the faculty of this body to absorb the calorific principle may be lessened, by forcibly diminishing the magnitudes of its pores, and this essential point of the theory may be confirmed by experiment; for the specific gravity of a slip of caoutchouc is increased, by keeping it extended while it is weighed in water.

JOHN GOUGH.

VIII. *Remarks on Urine, and the Prognostics which are derived from it. By Dr.* Collenbuch*.

The superstitious ideas and erroneous opinions of the multitude, and even of many persons otherwise well informed, but ignorant of medicine, have made me think that it would be more useful to explain to that part of the public who are strangers to the healing art, the phænomena which are produced, whether in a state of health or sickness, than to make known empirical rules and modes of treatment, which are more dangerous than useful.

This object has been already partly fulfilled in different physiological and anatomical treatises, destined for those who are not physicians; but let me only say a few words on a subject on which too many persons have false ideas, viz. on urine, and the prognostics derived from it.

The kidneys are destined to secrete from the blood the fluid known by the name of urine, and which cannot remain in the blood without prejudice to the health. The constituent parts of this fluid are different according to the age of the individual, the season of the year, food, drink, and other circumstances; but in general the urine contains what is called salt of urine, a little common salt, some calcareous earth, and mucus. These substances, according to circumstances, are dissolved in more or less water. The colour, smell, and taste of urine vary according to the proportion of these ingredients to the quantity of water. Thus, if suitable quantities of salt, lime, &c., are dissolved in a proportionate quantity of water, the result will be urine of a citron-yellow, and which has a particular smell and taste, such as is found in the urine of a person in a state of health; but if the proportion of water is too great relatively to other substances, the colour of the urine becomes clearer, and the smell and taste weaker. Supposing a contrary mixture, i. e. if these substances be dissolved in a smaller quantity of water, the urine takes a darker colour, a stronger taste and smell, and it will become more yellow, and of a deep or clear red. If it contains other substances, for example, a quantity of bile,

* From the German.

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its colour will become of a saffron-yellow; if the bile is corrupted it will become green, and black if there be any mixture of blood in the urine, as, for instance, in putrid fevers.

Thus it is from the appearance of the urine we are enabled to judge of the nature of the forces of the body, and of the different combinations of its constituent parts; and on that account we have recourse to that fluid for the purpose of determining the state of the forces and humours of a patient.

1. Let us judge if these forces are active or not. We call *forces* those qualities of the body by which it performs its functions in the order and manner that are convenient. These forces may be more or less active. If we wish to give action to a force which is rather inert, we excite it by irritation. For example, when we feel a tickling in the soles of the feet, the toes and the foot immediately contract: since the skin or the fibres have this power of contracting themselves by the influence of irritation, it thence results, that the veins, which every where traverse the body, shrink, and consequently can only conduct a smaller quantity of the blood—merely the aqueous particles; and if a similar contraction takes place in the region of the kidneys, they will receive only the aqueous parts of the blood, and will secrete a clear urine like water. This is what takes place in fevers when shivering fits are experienced. But this quality of urine, which in this case announces an augmentation of forces, may, under other circumstances, announce a weakness and relaxation of the body, particularly in the kidneys, which then make their secretions less perfectly. Such urine may indicate great danger, and even death. If it comes from persons attacked by inflammatory fevers, it announces that the blood is so thick, viscous, or near being coagulated, that drinks cannot mix with it, and are voided almost without alteration. I mean to state hereafter the consequences which result from this difference in prognostics by means of urine.

2. The urine makes us acquainted with the nature of the constituent parts of the body, and of their relative quantities. It enables us to judge also if those parts be strongly united together, or if they are very soluble, and tend to corruption.

In

In the latter case the urine will be of a deeper brown colour, and often black, and its smell will be as fœtid as the urine of a person in good health which has been allowed to stand a long time, especially if in a warm exposure. If the urine, then, at the moment of evacuation, has already acquired a fœtid smell, and if the colour of it is dark brown, it may be safely concluded that the humours of the body are extremely soluble, and have a tendency to putrefaction.

If, by accident, any one of the constituent parts of the humours is augmented; if the secretion of a part of them, which, being hurtful or useless to the body, ought to be separated from it, is arrested,—for example, the secretion of sweat; or if the humours destined for other functions (bile for instance) enter into the blood, it follows necessarily that the quantity of the constituent parts of the blood, and of course of the urine also, will be changed: and thus the urine will take other colours and other qualities.

Thus when perspiration is suppressed, at least if the matter which should be exuded does not recoil into another channel, the consequence will be an abundant discharge of clear and limpid urine.

If bile be mixed with the blood, supposing the bile to be in a good state the urine will become saffron yellow: if it be corrupted, the urine will become green or black.

If the urine is turbid, or of a clear brown, it indicates that the functions of the body are disturbed, and that secretion cannot be made in the proper manner. This urine, because it has a resemblance to those of certain animals, is called *urina jumentosa*.

The urine of itself never proves any thing positively: other circumstances of the malady must confirm the presumptions which the appearance of the urine had enabled us to form; and it is very wrong to examine first the urine, and the other symptoms of the disease afterwards. If the physician takes the uncertain signs of the urine as certain prognostics of the disorder, the patient may, in his turn, regard that as a certain sign of the ignorance of his physician.

To prove what I advance, I shall only cite an example. Black urine indicates corrupted bile; or with one who has strained

strained himself, or met with bruises or violent treatment, it indicates the absorption of coagulated blood. In putrid fevers it announces the dissolution of humours; it may also indicate the suppression of hæmorrhoides, or the courses: and in women in childbed, the suppression of the *lochia*.

But supposing that a man who has been severely beaten in some scuffle, or trampled under foot, attempts to heal himself by domestic medicines alone, but still feels a continuance of pains in his back and body; and that, after bathing the parts with wine, or using some similar remedy, he finds to his great surprise, at the end of three or four days, that his urine is black,—immediately he sends it to his physician and asks what is his disorder,—as if the physician could know it by divination. If the physician is not aware of the real cause of the disorder, he never will divine that the patient has received blows, that his back is marked with black and yellow spots, and that his body contains coagulated blood, of which a part, however, escapes into the urine.

He will more likely say, if putrid fevers are then raging, that the patient labours under that species of fever, and that he will die soon; or, perhaps, he will name some of the disorders above mentioned as giving this blackness to the urine, according as he finds more or less resemblance in the appearance of that fluid to the indications which we usually expect from it of various disorders.

IX. *Twenty-sixth Communication from Dr.* THORNTON, *relative to Pneumatic Medicine.*

To Mr. Tilloch.

No. 1, Hinde-street, Manchester-square, Feb. 18, 1806.

DEAR SIR,

HAVING saved the life of a first cousin, sir John Braithwaite, (who by mistake took a two ounce phial of laudanum for another draught,) by means of acids and the inhalation of vital air, I had confidence in this practice, and a few days back tried it in a very decided case.

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A gentleman in the vigour of life meeting with a great disappointment, had not the resolution to bear up against the shock, and he formed the horrid plan of self-murder. The pistol disappointing him in going off, he took two ounces of laudanum, which he had procured in smaller portions at different shops. The apothecary in the neighbourhood, Mr. Goodchild of Park-street, was sent for, who got down a scruple of vitriolated zinc, one of the quickest and most powerful of our emetics. Not finding this to act, he gave the same dose, and repeated it until a drachm was taken, with a scruple of tartar emetic; but such was the torpor of the stomach, that, as in the case of sir John Braithwaite, no emetic effect was produced. In this state I was sent for, and I found the patient, a gentleman in the army, of noble birth, convulsed; his eyes swimming in death; cold clammy sweats came on; the pulse scarcely perceptible, intermittent, and hardly beating after each convulsion, the head hung pendent upon the shoulder. As soon as I entered the house I ordered vinegar to be brought up, and lemon juice to be procured; and diluting the vinegar with a little water, pressing down the tongue, I poured the vinegar into the œsophagus, and afterwards I got down more than three lemons mixed with a little water. The effect of the acid was instantly perceptible; the eyes opened; the senses returned; the convulsion for the time ceased; and the stomach having imbibed the oxygen from the acid, which is according to Girtanner the irritable principle, or, as must be allowed, the *sine qua non* of irritability, it became sensible to the emetic, and the stomach disgorged its contents. More acid being got down, and plentiful dilution, what was thrown up was free from all acidity, and reason resumed its empire; but still a sense of great drowsiness prevailed, with occasional spasms. The *vital air* was now procured, which Mr. Ince of York Buildings administered; and the gentleman, after inhaling a gallon diluted with twice that quantity of atmospheric, expressed himself "greatly revived."—This being repeated gave the muscles their tone, the spasms recurred with diminished violence, and the patient "felt less languor." To be brief: he passed a restless

a restless night, slept towards morning; awoke with headache; took his breakfast of coffee in bed; got up to dinner, and was as well as ever the following day.

Observations on this Case by Dr. Thornton.

1. Opium deprives the system of its oxygen; that is, it renders the fibres less attractive of that principle: and hence oxygenated metals, presenting less affinity to the fibres of the stomach, are not decomposed, and fail of counteracting the fatal effects of opium. The vegetable acids easily give out their oxygen; that is, the elective attraction is greater here, and they, therefore, at once counteract the effects of laudanum.

2. I have had two cases where this poison has been taken into the stomach, and tried acids with the same marked benefit; and the bad effect of the laudanum was soon removed, especially when the vital air has been inhaled.

3. I am called by Dr. Rowley "*vital air and gas mad*;" but stand not alone, as Dr. Beddoes assures me, in a letter last week received, "that he continues to order the different airs in various diseases, of which practice he entertains the same favourable opinion as ever."—Dr. Rowley calls my refutation of his cases* personal scurrility and abuse:—can any thing be more so than branding regular physicians with the appellation of *madness?*

I remain, sir,
Your obedient humble servant,
ROBERT JOHN THORNTON.

X. *On a new Black Dye to be applied to all Kinds of Linens and Stuffs. By Mr.* HERMBSTAEDT, *Berlin*†.

THE black colours which are commonly applied to linen and cotton stuffs are composed of iron and vinegar. Their base is always oxide of iron, which is mixed with decoctions

* My new work, in answer to Dr. Rowley, is entitled Vaccinæ Vindicia, or A Vindication of the Cow-pock.

† From *Biblioth. Phys. Economique*, an xiv. no. 2.

of wood of different kinds. All these colours incline either to red or blue, and they resist but feebly the action of the air, of water, and of acids. The tincture which I have composed, and which I use daily in dyeing all kinds of cotton, silk, or wool stuffs to an unalterable black, embraces an intimate union of the oxide of iron with that of copper and the pyro-ligneous acid.

Preparation of the Pyro-ligneous Acid.

Take a tubulated retort made of plate iron, or of cast iron, which is better, place it in a furnace in such a manner that the neck be perfectly free, and the bottom receive directly the heat of the fire. It must be luted carefully, and there must be introduced into the retort some chestnut wood cut into small bits. The distillation then commences with a very moderate fire, which is progressively increased till no more liquid passes into the receiver. The acid which is found in the receiver, mixed with a kind of oil, may be separated from it by a filter of gray paper: the wood will be reduced to charcoal in the retort.

Preparation of the Oxide of Iron.

Dissolve 4 pounds of vitriol (sulphate) of iron, very pure, in 24 pounds of rain water. Dissolve in like manner 4 pounds of potash in 12 pounds of filtered water. These two solutions, when well mixed, will appear at the beginning of a deep green; but in a little time the surface exposed to the air will take a dark red colour; then pour the whole on a filter of linen: the oxide which will remain after the water has passed ought to be washed in a great deal of water, to free it from all adhering salt. Leave this oxide exposed on a plate to the action of the atmosphere, which will reduce it to a state of red oxide.

Preparation of the Oxide of Copper.

To prepare this oxide, take a pound of blue vitriol of Cyprus (sulphate of copper), which dissolve in 12 pounds of rain water: make it boil, and mix with it a pound of water saturated with potash, and you will obtain a green precipitate, which must be well washed after being filtered.

Preparation

Preparation of the Mordant for Black.

Take three parts of the oxide of iron and one part of the oxide of copper: triturate them in a marble mortar, and pour on them the necessary quantity of pyro-ligneous acid to dissolve them. Filter the whole to separate the thick parts, and the mordant is made.

Application of this Mordant in dyeing Black.

Steep the stuffs intended to be dyed in this mordant thickened at pleasure. Afterwards proceed to the dyeing in the ordinary decoctions made with different dye-woods. The colour obtained will be a very beautiful black, and almost unchangeable by all chemical agents. If the black is meant to serve for printing stuffs or cloths, it is thickened very much, and by mixing it with different tinctures of dye-wood, it will form a black equally beautiful and lasting.

XI. *New Experiments on the Respiration of Atmospheric Air, chiefly with respect to the Absorption of Azote and the Respiration of Gaseous Oxide of Azote. By Professor* PFAFF, *of Kiel**.

THE great discoveries in pneumatic chemistry, and the applications, as ingenious as useful, which have been made of those discoveries to explain the phænomena of the œconomy of organized beings (above all, of animal œconomy), and the careful researches of the most distinguished naturalists, have contributed much to clearing up the doctrine of the chemical effects of respiration. There has been a general agreement among physiologists on some of the most essential points of this theory, as well as upon the production of carbonic acid, the employment of oxygen gas, and the production of animal heat which depends on it. But, notwithstanding the zeal of naturalists, there are still some points which are not sufficiently clear; and the want of agreement in the results of experiments on those points proves that it was necessary

* From the *Annales de Chimie*, No. 164.

to make new efforts to remove those errors, and to discover also their sources. The researches of the celebrated Davy have happily succeeded for this end; and his "Researches Chemical and Philosophical, chiefly concerning Nitrous Oxide, &c. &c." form, in this point of view, a new epoch in the chemical doctrine of respiration. The celebrated editors of the *Bibliotheque Britannique* have shown that they appreciated the value of these researches by giving an extract from them, very minute and instructive, in their excellent journal *; and the manner in which Berthollet has given us another extract in the *Annales de Chimie* ought to fix the attention of all chemists on the work of Mr. Davy. The differences that were found in the results of preceding experiments on the quantity of carbonic acid produced in the act of respiration, were less important, and might depend entirely on the difference of vital powers in different individuals who have been made subjects of these experiments; and in this point of view, a revision of these experiments was of less consequence. But the part which azotic gas performs in the act of respiration had been too little appreciated by chemists. It had been common to attribute to it a purely passive part. Goodwyn alone believed that he had observed a very considerable absorption of azotic gas; but his experiments had not been made with all due exactness, and were too much in contradiction with the experiments of Lavoisier, Seguin, Abernethy, Fothergill, Menzies, &c., to command attention. The experiments made on the slow combustion of phosphorus, which did not succeed in pure oxygen gas, and which is so much favoured by the concurrence of the azotic gas of the atmospheric air, might serve to guide us in determining the service which this great quantity of azote renders in respiration. And a notice, unfortunately too short, of the results of the last experiments of the immortal Lavoisier upon respiration, supports this view of the subject. According to his results, a greater quantity of oxygen gas was decomposed in the same time by the respiration of the atmospheric air than by the respiration of

* *Vide* tom. xix. xx. xxi. in the department of Arts and Sciences.

oxygen

oxygen gas. But hitherto we had only had probabilities, or results of which the solidity could not be properly ascertained. It is to Davy that we owe an exact and well founded knowledge of the active part which azotic gas performs in the act of respiration. But the more that these results were new and interesting, the more they deserved to be confirmed by the agreement of the experiments of other chemists; and it was from this view that I undertook, last winter, a series of experiments on the respiration of atmospheric air, and the respiration of the gaseous oxide of azote, of which I submitted the principal results to the National Institute.

Experiments on the Respiration of Atmospheric Air and of Oxygen Gas.

These experiments have been undertaken in the academical laboratory of the university of Kiel, (which is provided with all the exact apparatus of modern chemistry,) for the most part in common with my scholars, and particularly with one of them of the name of Dierks, who made himself most frequently the subject of experiments.

That I might determine with exactness the changes which the atmospheric air undergoes by respiration, and be able to decide the absorption of azotic gas, it was necessary to set out, as from a solid foundation, with the diminution which a determined volume of atmospheric air experiences by respiration. It is therefore the first point to be determined by exact experiments.

1. There were respired 170 cubic inches of air (Paris measure) from one of the great reservoirs of a gasometer constructed at Paris (after the model of that of Charles, upon water covered with oil, to hinder the absorption of carbonic acid gas produced by respiration), once in the space of from 10 to 12 seconds. The diminution was 4·72 cubic inches, $= \frac{1}{36}$ of the primitive volume. This experiment, repeated twenty times in the same manner, afforded the same result.

2. 144 cubic inches were respired in the space of from 10 to 12 seconds; the diminution was 4 inches, $= \frac{1}{36}$ of the whole volume.

3. The same volume was respired twice during 22 seconds. The diminution amounted to 8 cubic inches, $= \frac{1}{18}$ of the

primitive volume. The same volume, having been respired three times during 30 seconds, the diminution amounted to 12 cubic inches, $= \frac{1}{12}$ of the primitive volume.

4. Sixty cubic inches were respired three times during 25 seconds: the diminution was 6 cubic inches, $= \frac{1}{10}$ of the primitive volume.

5. 170 cubic inches were respired four times during one minute: the diminution amounted to 20 cubic inches. This experiment was repeated several times. The diminution was almost always the same, 18, 19, 21, or 20 cubic inches, $= \frac{2}{17}$.

6. 168 cubic inches being respired during 50 seconds, at four great and four small respirations, were diminished by 14, $= \frac{1}{12}$ of their primitive volume.

7. 430 cubic inches underwent, at twelve respirations in 90 seconds, a diminution of 24, $= \frac{1}{18}$.

These results agree very well with those obtained by Mr. Davy on the diminution of air by respiration. Davy found the diminution by a single respiration $= \frac{1}{43}$. By respiration continued for a minute, the diminution was $= \frac{1}{18}$.

The greatness of the diminution does not depend alone on the time during which a given volume of air is respired, but principally on the greatness of the volume itself: and this diminution ought to be relatively as much less as the volume of air that is inhaled is greater. There is undoubtedly a very essential error in the conclusions of Abernethy, inasmuch as he assigned to the expired air a greater volume than to that which was inhaled. And the calculations of Goodwyn rest upon very untenable grounds, since he supposes the volumes of both to be equal.

To determine comparatively the diminution of oxygen gas by respiration, 170 cubic inches of oxygen gas, drawn from manganese, were respired in the same manner, and under the same circumstances, as the above (*vide* 5.) 170 cubic inches of atmospheric air. The diminution amounted to 30 cubic inches; in the other experiments to 33, 29, 31: thus, by a mean term, to $\frac{2}{11}$ of the primitive volume.

This diminution, being established in a precise manner, served as a basis for determining the absorption of azotic gas.

8. Eighty cubic inches were respired once, and slowly, during

during a space of from 10 to 12 seconds; and the middle quantity of the expired air was received over mercury. In this respired air the constituent parts in a hundred were 4·16 of carbonic acid gas; 16·55 of oxygen gas absorbed by slow combustion of phosphorus; and 79·19 of azotic gas. An eudiometrical experiment, made at the same time, gave the following proportion in the atmospheric air: 1 carbonic acid, 21 oxygen gas, 78 azotic gas. The total diminution of the air was, according to the preceding experiments (1.) $= \frac{1}{36}$. We find, therefore, the true quantity of azotic gas by the following proportion: $36 : 35 = 79{\cdot}19 : x = 76{\cdot}99$.

By subtracting these 76·99 from 78, the primitive quantity of azotic gas, we have a deficiency of 1·01; the volume of air before respiration being supposed to be divided into 100 parts. As the quantity of air inspired was = 80 cubic inches, the absolute diminution of azotic gas by a single respiration is found by this proportion, $100 : 80 = 1{\cdot}01 : x = 0{\cdot}808$ cubic inch.

9. In another experiment 60 cubic inches were respired once in the space of from 10 to 12 seconds, and the latter portion of expired air was received over mercury. The proportion of constituent parts in 100 were, 4·68 of carbonic acid gas, 17·68 of oxygen gas, 77·74 of azotic gas. An eudiometrical experiment made at the same time, gave, in 100 parts, 1 carbonic acid, 22 oxygen gas, 77 azotic gas. To find the true quantity of azotic gas it is necessary to diminish the 77·74 of azotic gas by $\frac{1}{36}$, which gives, by proportion, $36 : 35 = 77{\cdot}74 : x$. This quantity = 75·58, subtracting the quantity of azotic gas before respiration, leaves a loss of 1·42, supposing the volume of air to be divided into 100 parts. The true diminution is therefore found by the following proportion, $100 : 60 = 1{\cdot}42 : x = 0{\cdot}852$ cubic inch.

10. Thirty cubic inches were respired, in the same manner, three times during 16 seconds. The air which was exspired contained, in 100 parts, 5 of carbonic acid gas, 14·5 of oxygen gas, 80·5 of azotic gas. The atmospheric air contained, according to an eudiometrical experiment made at the same time, 1 of carbonic acid gas, 20·975 of oxygen

gas, 80·025 azotic gas. We have, then, by the following proportion, 12 : 11 = 80·5 : 73·79; the diminution = 4·235. And in cubic inches by proportion, 100 : 30 = 4·235 : x = 1·2705.

These experiments, which were frequently repeated, and always gave the same result, establish, therefore, completely the absorption of azotic gas in the act of respiration, and the active part which this gas performs. We understand, at the same time, more easily why the azotic gas, comparatively with other mephitic gases, is so little adverse and pernicious to our lungs. According to the experiments of Lavoisier and Seguin, animals supported life very well in a mixture of 15 parts of azotic gas with 1 of oxygen; although the same animals were suddenly suffocated in a mixture of 40 parts of oxygen gas, 45 of azotic gas, and 15 of carbonic acid gas. We understand, at least in some degree, the extraordinary effects of the gaseous oxide of azote. We understand the transformation of chyle, being less animalized, less azotized in the lymphatic part of the blood, to be very much animalized and very much azotized by the act of respiration, &c. However, the quantity of azotic gas absorbed by a single respiration is not very considerable; which agrees perfectly with the experiments of Davy, who, in nineteen respirations of a volume of 161 cubic inches, absorbed no more than 5·1 cubic inches of azotic gas.

11. To determine the quantity of carbonic acid gas produced by the respiration of atmospheric air, 60 cubic inches were respired once during 10 or 12 seconds, and received over the mercury in the expiration. Lime water absorbed 4·68 in 100 parts. This experiment, several times repeated, gave the same result. The latter portion of expired air, transmitted frequently through lime water, exhibited a diminution of 4·9 in 100 parts.

12. Twenty cubic inches, three times respired successively during 10 seconds, showed no more than $\frac{5}{100}$ of carbonic acid gas.

13. 170 cubic inches were respired four times during 50 seconds, and there was obtained $\frac{5}{100}$ of carbonic gas.

14. 170

14. 170 cubic inches were respired from a bladder eight times in a minute. The lime water absorbed $\frac{8}{100}$.

This quantity of carbonic acid produced by respiration gave a term of comparison to determine the quantity of decomposition of oxygen gas in the respiration of the same quantities of atmospheric air and of pure oxygen. The preceding experiment (7.) had denoted that the diminution of oxygen gas was more considerable than that of atmospheric air. According to this, one might expect, with the appearance of probability, that the production of carbonic acid gas should be also more considerable; which was confirmed by direct experiments.

15. 170 cubic inches of oxygen gas obtained from manganese were respired four times during 50 seconds: the diminution was 30 cubic inches. The quantity of carbonic acid which was produced amounted to $\frac{8}{100}$. The atmospheric air, respired in the same manner, and under the same circumstances (13.), contained only $\frac{5}{100}$ of carbonic acid.

16. Seventy cubic inches, respired from a bladder in the space of 50 seconds, even gave $\frac{3}{100}$ of carbonic acid.

Experiments on the Respiration of Gaseous Oxide of Azote.

The gaseous oxide of azote was obtained, according to the process of Davy, from crystallized nitrate of ammonia. This nitrate of ammonia gives products very different in different temperatures. I undertook on this subject an important operation, which I submitted to the inspection of the National Institute. I have only to remark, by the way, that we obtain at the commencement oxygenated muriatic gas, if the nitrate of ammonia is not entirely free from muriatic acid: that at a temperature which does not go beyond the 220th degree of the centigrade thermometer we obtain the gaseous oxide of azote in very great quantity, and very pure, and without a mixture of those white vapours which have the taste of mustard; but at higher temperatures, and above all at a red heat, the gaseous oxide of azote is not disengaged, but there are formed nitrous gas and white vapours, quite

quite uncommon in the analysis of which I am at present engaged.

To prevent all explosion, I constantly mix the nitrate of ammonia with very pure sand. To obtain the gaseous oxide of azote very pure, the distillation is performed in a sand bath, and the fire is managed with great care. When the whole succeeds well, the gas is so pure that one may respire it immediately. It has an agreeable taste, almost saccharo-vinous. If it is mixed with the white vapours produced by an excess of heat, it must be left a sufficient time to separate itself. After two or three hours it may be respired without danger. The effects which Davy has observed, and which Pictet has described in so interesting a manner in his second letter (Bibl. Britannique, tome xvii. p. 406.), have been perfectly confirmed in my experiments. Many persons who have respired this gas have been intoxicated very quickly, and put into a state of ecstasy very extraordinary and very agreeable. Others have resisted its effects a little more; and one individual was not at all affected by it. This state of intoxication has always passed without leaving a sensible relaxation. I am still continuing these experiments. It is not improbable that this gas may become a very important remedy for melancholic persons. I will not fail to communicate the whole series of experiments to the National Institute.

XII. *Additional Experiments and Remarks on an artificial Substance which possesses the principal characteristic Properties of Tannin.* By CHARLES HATCHETT, *Esq. F. R. S.*

[Continued from our last volume, p. 326.]

§ IV.

I MADE several unsuccessful attempts to form the artificial substance by means of oxymuriatic acid; and it therefore appeared certain, that although a variety of the tanning matter could be produced by the action of sulphuric acid on resinous substances, yet the most effective agent was nitric

nitric acid, which readily formed it when applied to any sort of coal.

But I nevertheless suspected, that possibly this substance, or something similar to it, might be produced without absolutely converting vegetable bodies into coal; for it seemed, as I have observed in my former paper, that this only served to separate the carbon in a great measure from the other elementary principles (excepting oxygen) which were combined with it in the original substance, and thus to expose it more completely to the effects of the nitric acid, as well as to prevent the formation of the various acid products, which are so constantly afforded by the organized substances when thus treated. At first I had some thoughts of employing touchwood in this experiment: but not being able immediately to procure any, it occurred to me that indigo might probably answer the purpose; for from some experiments made by myself, as well as from those described by Bergman*, I well knew that the proportion of carbon in this substance is very considerable. The following experiment was therefore made:

1. On one hundred grains of fine indigo which had been put into a long matrass, one ounce of nitric acid diluted with an equal quantity of water was poured; and as the action of the acid was almost immediate and extremely violent, another ounce of water was added. When the effervescence had nearly subsided, the vessel was placed in a sand-bath during several days, until the whole of the liquid was evaporated.

On the residuum, which was of a deep orange colour, three ounces of boiling distilled water were poured, by which a considerable part was dissolved.

The colour of the solution was a most beautiful deep yellow, and the bitter flavour of it surpassed in intensity that of any substance in my recollection; it was examined by the following reagents:

Sulphate of iron produced a slight pale yellow precipitate.

Nitrate of lime only rendered it a little turbid; after which

* *Analysis Chemica Pigmenti Indici.* Opuscula Berg. tom. v. p. 86.

a small

a small portion of white powder subsided, which had the characters of oxalate of lime.

Muriate of tin produced a copious white precipitate, which afterwards changed to a yellowish-brown.

Acetite of lead formed a very beautiful deep lemon-coloured precipitate, which possibly may prove useful as a pigment.

Ammonia rendered the colour much deeper; after which the liquor became turbid, and a large quantity of fine yellow spiculated crystals was deposited, which being dissolved in water did not precipitate lime from its solutions.

The flavour of these crystals was very bitter, and I suspect them to be composed of ammonia combined with the bitter principle first noticed by Welther*.

Lastly, when dissolved isinglass was added to the yellow solution of indigo, it immediately became very turbid, and a bright yellow substance was gradually deposited, and coated the sides of the glass jar with a tough elastic film, which was insoluble in boiling water, and possessed the characters of gelatine combined with tanning matter.

By this experiment I therefore ascertained, that a variety of the artificial tanning substance could be formed without previously converting the vegetable body into coal; and I have since discovered, that although indigo more readily yields this substance than most of the other vegetable bodies, yet in fact, very few of these can be regarded as exceptions, when subjected to repeated digestion and distillation with nitric acid.

2.—A. In my former paper I have stated, that common resin, when treated with nitric acid, yielded a pale yellow solution with water, which did not precipitate gelatine, and that it was requisite to develop part of the carbon in the state of coal by sulphuric acid, before any of the tanning substance could be produced; but having again made some of these experiments, I repeated the abstraction of nitric acid several times, and then observed, that the solution of resin in water acted upon gelatine similar to the solution of indigo

* Thomson's System of Chemistry, 2d edit. vol. iv. p. 216.

which

which has been described, and formed a tough yellow precipitate, which was insoluble in boiling water.

With other reagents the effects were as follow:

Sulphate of iron, after 12 hours, formed a slight yellow precipitate.

Nitrate of lime did not produce any effect.

Muriate of tin, after 12 hours, afforded a pale brown precipitate.

And acetite of lead immediately formed a very abundant precipitate of a yellowish white colour.

I repeated this experiment on common resin, and remarked, that during each distillation nitrous gas was produced, whilst the strength of the acid which came over was diminished: the cause therefore of the change in the properties of the resin seemed to me very evident, and I was induced to extend the experiments to various resinous and other substances; but as the process was uniformly the same, I shall only notice the principal effects.

B. Stick lac, when separated from the twigs, and treated as above described, copiously precipitated gelatine.

C. Balsam of Peru during the process afforded some benzoic acid, and gelatine was precipitated by the aqueous solution.

D. Benzoin also, after the sublimation of some benzoic acid, yielded a residuum, which with water formed a pale yellow solution of a very bitter flavour.

This solution with sulphate of iron afforded a slight pale yellow precipitate.

With nitrate of lime not any effect was produced.

The solution with muriate of tin became turbid, and a small quantity of brownish-white precipitate subsided.

Acetite of lead immediately produced a copious pale yellow precipitate.

And solution of isinglass formed a dense yellow precipitate, which was insoluble in boiling water.

E. Balsam of Tolu, like balsam of Peru, and benzoin, afforded some benzoic acid; and the residuum being dissolved in water was found to precipitate gelatine.

F. As the results of the experiments on dragon's blood were

were somewhat remarkable, I shall here more particularly relate them. One hundred grains of pure dragon's blood, reduced to powder, were digested in a long matrass with one ounce of strong nitric acid; the colour immediately changed to deep yellow, much nitrous gas was evolved, and to abate the effervescence one ounce of water was added. The digestion was continued until a deep yellow dry mass remained; and the matrass being still kept in the sand-bath, a brilliant feather-like sublimate arose, which weighed rather more than six grains, and had the aspect, odour, and properties of benzoic acid*.

The residuum was of a brown colour, and with water formed a golden yellow-coloured solution, which by nitrate of lime was not affected.

With sulphate of iron it afforded a brownish-yellow precipitate.

With muriate of tin the result was similar.

With acetite of lead a lemon-coloured precipitate was produced.

Gold was precipitated by it in the metallic state, whilst the glass vessel acquired a tinge of purple:

And dissolved isinglass produced a deep yellow deposit, which was insoluble in boiling water.

A portion of the same dragon's blood was simply exposed to heat in the same matrass, but not any appearance of benzoic acid could be discovered. I am therefore induced to believe, that in the first experiment it was obtained as a product, and not as an educt: a fact which as yet has not been suspected.

G. Gum ammoniac afforded a brownish yellow solution, the flavour of which was very bitter and astringent.

By sulphate of iron, this solution only became of a darker colour, but did not form any precipitate.

* According to these experiments, dragon's blood ought to be arranged with benzoin and the balsams; but as the samples of this drug are not always precisely similar, it would be proper to examine every variety. That which was employed in my experiments, was a porous mass of a dark red, and was sent to me by Messrs. Allen and Howard, of Plough-court, in Lombard street.

Nitrate

Nitrate of lime rendered it turbid, and produced a slight precipitate.

Muriate of tin formed a copious yellow precipitate.

Acetite of lead produced a similar effect:

And gelatine yielded a bright yellow deposit, which was completely insoluble by boiling water.

H. Asa fœtida yielded a solution which also precipitated gelatine like the substances above described.

I. Solutions of elemi, tacamahac, olibanum, sandarach, copaiba, mastich, myrrh, gamboge, and caoutchouc, were next examined; but these, although they precipitated the metallic solutions, did not affect gelatine. It is possible, however, that they might have produced this effect, had they been subjected to a greater number of repetitions of the process.

K. Sarcocol, in its natural state, as well as the gum separated from it by water, when treated with nitric acid, did not precipitate gelatine; but produced effects on the metallic solutions similar to the above-mentioned substances.

L. Gum arabic afforded oxalic acid, but not any of the tanning matter.

M. Tragacanth yielded an abundance of saclactic acid, of oxalic, and of malic acid, but not the smallest vestige of the artificial tanning substance.

N. Manna, when treated with nitric acid in the way above described, afforded oxalic acid, part of which was sublimed in the neck of the vessel.

The residuum with water formed a brown solution, which yielded a pale yellow precipitate with sulphate of iron.

Muriate of tin produced a pale brown precipitate.

Acetite of lead formed one of a brownish-white hue.

Lime was copiously precipitated from the nitrate of lime in the state of oxalate; but not the smallest effect was produced on solution of isinglass.

O. Liquorice however afforded a different result; for although the solution after the process with nitric acid resembled in appearance that which was yielded by manna, yet the effects were not the same.

Sulphate

Sulphate of iron, after twelve hours, produced a slight brown precipitate.

Muriate of tin had a similar effect.

Acetite of lead formed a brownish-red deposit.

Nitrate of lime also occasioned a brown precipitate:

And solution of isinglass rendered it very turbid, and produced a yellowish-brown precipitate, which was insoluble in boiling water, and possessed all the other characters of gelatine combined with the tanning substance.

P. Guaiacum, the properties of which are so singular in many respects, afforded results (when treated with nitric acid in the manner which has been described) different from the resins, although its external and general characters seem to indicate that it appertains to those bodies.

Nitric acid acted upon it with great vehemence, and speedily dissolved it. The residuum which was afterwards obtained, was also found to be almost totally soluble in water, and the solution acted on the metallic salts like those which have already been noticed; but with gelatine it formed only a very slight precipitate, which was immediately dissolved by boiling water; and the remainder of the solution being evaporated, yielded a very large quantity of crystallized oxalic acid; so that in this respect guaiacum was found to resemble the gums, and to be totally unlike the resins*.

§ V.

As many vegetable substances when roasted yield by decoction a liquid which in appearance much resembles the artificial tanning matter when dissolved in water, I roasted some of the common dried peas, horse-beans, barley, and wheat flour, the decoctions of which however did not afford any precipitate by solution of isinglass.

* The properties of guaiacum which have been described, as well as those which were previously known, appear to indicate, that it is a peculiar substance of a nature distinct from the resins, balsams, and even the gum resins.

So remarkable indeed is this substance, that an accurate series of experiments on the whole of its properties may justly be placed amongst the chemical desiderata.

Even the decoction of coffee did not yield any precipitate by this method, until several hours had elapsed, and I found that the precipitate so formed was permanently soluble in boiling water. But to explain this, we must recollect, it is extremely probable, that some peculiar nicety is required in the roasting of such bodies before the tanning substance can be developed; and this seems to be corroborated by some experiments which I made on the decoction of a sort of coffee prepared from the chicorée (I suppose endive) root, which was given me by Sir Joseph Banks; for although this decoction did not afford an immediate precipitate with solution of gelatine, and although the precipitate was also apparently dissolved by boiling water, yet upon cooling, the same precipitate was reproduced in its original state. I am therefore inclined to believe, that the tanning substance is really developed in many of the vegetable bodies by heat, but that a certain degree of temperature, not very easy to determine, is absolutely requisite for this purpose.

Before I conclude this section, it may be proper to observe, that when a small quantity of nitric acid was added to any of the above-mentioned decoctions, and when these had been subsequently evaporated to dryness, and afterwards dissolved in distilled water, they were converted into a tanning substance perfectly similar to that which is produced by the action of nitric acid on the varieties of coal.

§ VI.

In the preceding paper, a variety of the tanning substance was slightly noticed, which was formed by the action of sulphuric acid upon common resin, elemi, amber, &c. &c. and as an instance has occurred of the formation of the same substance from camphor, accompanied by circumstances which tend to increase our knowledge of the properties of the latter, I shall here describe this experiment.

Experiment on Camphor with sulphuric Acid.

The effects produced on camphor by sulphuric acid have been but very superficially examined; for all that has hitherto been stated amounts to this, that camphor is dissolved by

sulphuric acid, that a brown or reddish-brown solution is formed, and that the camphor is precipitated unchanged from this solution by water. These facts, however, only relate to a certain period of the operation; for, if this be long continued, other effects are produced, which I shall now describe.

A. To one hundred grains of pure camphor put into a small glass alembic, one ounce of concentrated sulphuric acid was added. The camphor immediately became yellow, and gradually dissolved, during which the acid progressively changed to brownish-red, and afterwards to brown. At this period scarcely any sulphureous acid was evolved, but in about one hour the liquid became blackish-brown; much sulphureous acid gas was then produced, and continued to increase during four hours, when the whole appeared like a thick black liquid, at which period not any odour or appearance of camphor could be perceived, but only that of the sulphureous acid. After two days, during which time the alembic had not been heated, there did not appear any alteration, unless that the production of sulphureous gas was much diminished. The alembic was then placed in a sand-bath moderately warm, by which more of the sulphureous gas was obtained, but this also soon began to abate. After the lapse of two other days, I added gradually six ounces of cold water, by which the liquid was changed to reddish-brown, a considerable coagulum of the same colour subsided, the odour of sulphureous gas, which in some measure had still prevailed, was immediately annulled, and was succeeded by one which resembled a mixture of oils of lavender and peppermint.

The whole was then subjected to gradual distillation, during which the water came over strongly impregnated with the odour above mentioned, accompanied by a yellowish oil, which floated on the top of it, and which, as far as could be ascertained, amounted to about three grains.

B. When the whole of the water was come over, there was again a slight production of sulphureous gas. I then added two ounces of water, which I drew off by distillation, but did not obtain any of the vegetable essential oil which has

has been mentioned, nor did the odour of it return. I therefore continued the distillation until a dry blackish brown mass remained; this was well washed with warm distilled water, by which, however, nothing was extracted; but when two ounces of alcohol were digested on it during twenty-four hours, a very dark brown tincture was formed.

The residuum was digested with two other ounces of alcohol in like manner, and the process was repeated until the alcohol ceased to act.

The residuum had now the appearance of a compact sort of coal in small fragments, it was then well dried, and after exposure to a low red heat in a close vessel weighed fifty-three grains.

C. The different portions of the solution formed by alcohol were added together, and being distilled by means of a water-bath, a blackish brown substance was obtained, which had the appearance of a resin or gum with a slight odour of caromel, and weighed 49 grains.

The products therefore which were thus obtained from 100 grains of camphor when treated with sulphuric acid, were,

	Grains.
A. An essential oil which had an odour somewhat resembling a mixture of lavender and peppermint, about	3
B. A compact and very hard sort of coal in small fragments	53
C. And a blackish brown substance of a resinous appearance	49
	105

From this statement it appears, that there was an increase in the weight amounting to five grains, which I attribute partly to oxygen united to the carbon, and partly to a portion of water so intimately combined with the last product, that it could not be expelled from it by heat without subjecting it to decomposition. The properties of this substance were as follows:

1. It was extremely brittle, had somewhat of the odour

of caromel, the flavour was astringent, and it speedily dissolved in cold water, and formed with it a permanent dark brown solution.

2. This solution yielded very dark brown precipitates by the addition of sulphate of iron, acetite of lead, muriate of tin, and nitrate of lime.

3. Gold was copiously precipitated by it from its solution in the metallic state; and,

4. By solution of isinglass, the whole was completely precipitated, so that, after three or four hours, a colourless water only remained.

The precipitate was nearly black, and was insoluble in boiling water; from which property, as well as from the effect produced upon prepared skin by the solution, it was evident that the substance thus obtained from camphor was a variety of the artificial tanning matter, much resembling that which may be obtained from resinous bodies by means of sulphuric acid. But it must be observed that this sort of tanning substance seems to act less powerfully on skin than that which is prepared from carbonaceous substances by nitric acid, and the precipitate which the former produces with solution of gelatine is more flocculent and less tenacious than that which in like manner is formed by the latter.

It is however remarkable, that when a small quantity of nitric acid was added to the solution of the substance obtained from camphor, and when, after evaporating it to dryness, the residuum was dissolved in water, a reddish brown liquid was formed, which acted in every respect similar to the tanning substance obtained from the varieties of coal by nitric acid.

[To be continued.]

XIII. *On*

XIII. *On the Oxides of Gold, Tin, and other Metals; with Hints for extending their Uses in Dyeing. Communicated in a Letter to M.* Berthollet *by M.* John Michael Haussman *.

I believe that I am as well founded as yourself in admitting of intermediate terms of oxidation between the minimum and maximum of several metallic substances. I shall, by-and-by, cite an example of the oxide of tin at the minimum precipitated from its muriatic solution, and redissolved by an excess of caustic potash; the alkaline metallic solution of which I have already mentioned in my observations upon the Adrianople red, inserted in the *Annales de Chimie* †, as well as in another memoir upon the coloured oxides of tin, given to the public in the *Journal de Physique*. In avoiding to dilute the muriatic solution of tin too much with water, and employing a solution of caustic potash very much concentrated, a considerable quantity of calorie disengages itself during the mixture of these two liquors: one portion of the tin precipitates itself in the metallic state, while the other remains in solution in a state of intermediate oxidation. This alkaline solution has so strong an affinity for oxygen, that it changes into a gray colour the yellow oxide of gold fixed upon a piece of cotton by means of ammonia, while a similar yellow piece does not change its colour when allowed to soak in a pure liquor of caustic potash. The same change takes place upon plunging a small piece of cotton, which had been saturated with a solution of gold, well squeezed and dried, into an alkaline solution of tin. The same effect will follow if we pour into this alkaline solution a solution of gold diluted with water.

The change of the yellow colour of the oxide of gold by the alkaline solution of tin is not the only proof of the intermediate oxidation: this liquor, besides, possesses the property of extracting the blackish brown colour of the oxide of manganese fixed upon a piece of cotton by an alkaline pre-

* From the *Annales de Chimie*, No. 166.

† See Phil. Mag. vol. xii. and vol. xviii.

 cipitate,

cipitate. All these changes will take place more rapidly if, before performing the precipitation and re-solution by the liquor of caustic potash, the muriatic solution of tin is diluted with six or eight parts of water. In this case there will be no perceptible extrication of caloric, and there will be no precipitation of tin in a metallic state. This solution, the oxidation of which approaches to the term of minimum, preserves in general, without any precipitation of oxide, an aqueous transparency. Exposed for a long time to the atmospheric air, it does not lose the property of changing the yellow oxide of gold into gray, and of extracting the blackish brown colour of the oxide of manganese fixed upon a piece of cotton.

The oxide of manganese may present itself under different degrees of oxidation. If we impregnate a piece of cotton cloth with a transparent solution of sulphate of manganese, it will preserve its whiteness in drying: upon plunging the piece of cotton cloth thus impregnated into a solution of potash in a state of carbonate or of causticity, it will, when washed, be changed into a brown colour by the action of the atmospheric air. This colour will acquire a very dark hue, almost approaching to black, upon being allowed to remain some time in an alkaline oxymuriatic solution. This oxygenated alkaline liquor will assume a purple colour, of a more or less transparent intensity, on leaving therein exposed, a longer or shorter time, some brown precipitate of manganese in place of a piece of cloth coloured with this metallic substance, which therein dissolves itself, being oxidated by the fluid.

In general, it is proper to attend to particular results in exposing all the metallic oxides to the action of this oxygenated muriatic alkaline liquor. This, perhaps, would be the means of giving them acid properties, and of proving at the same time more and more the gradual oxidation of most metals. Above all, it may be remarked of the white oxide of lead, which becomes gradually more and more coloured by a long exposure in this oxygenated liquor, in which it is necessary, nevertheless, to stir it often.

The muriatic and nitro-muriatic solutions of tin, very much

much diluted in water, have an aqueous transparency when they are well made; but when mixed together, a fine colour, like that of Malaga wine, is produced; a circumstance which could never happen, unless the oxygen of the nitro-muriatic solution united with the muriatic solution of tin. If, in a similar mixture, we pour by little and little, stirring it at the same time, the solution of gold with a great excess of acid, and diluted in 130 to 160 parts of water, the intensity of the colour of a similar mixture will increase more and more, and latterly present a very fine purple tincture, in which all sorts of stuffs may be dyed. By making the nitro-muriatic solution predominate, shades of peach flowers and lilies will be obtained; and, on the contrary, shades more or less gray are obtained by making the muriatic solution of tin predominate. Care must be taken, however, not to employ the latter in too great abundance, because, in powerfully extricating the oxygen from the oxide of gold, it will deoxidate it too much, and precipitate it. The precipitate produced in this case is not entirely deprived of oxygen, which hinders it from gilding silver in the cold, like the ashes produced from the combustion of a piece of cloth impregnated with a solution of gold. The longer or shorter preservation of the golden tincture will depend entirely upon the proportions of the two different solutions of tin, more or less overcharged with acids, and the solution of gold, in which the acid ought always greatly to predominate. In exposing the purple tincture of gold, of the most perfect transparency, to a strong heat, it will decompose itself, and precipitate the colour known by the name of purple of Cassius, the beauty of which will depend more or less on the nitro-muriatic solution of tin employed, which, mixed by itself with solution of gold, without the intervention of muriate of tin, produces no change of colour, and preserves itself a long time without forming any precipitate, if the mixture is not too much diluted with water. The purple tincture of gold is, properly, nothing else than the purple of Cassius, kept in solution by means of the oxygen of the nitro-muriatic solution of tin; and there is every reason to believe that in the purple of Cassius the oxide of gold is in some measure combined with

the oxide of tin, which, giving up to it its oxygen during its application to porcelain, hinders, in my opinion, the gold from reducing itself and regaining its metallic lustre. I cannot subscribe to the opinion of Dr. Richter, of Berlin, who asserts, in a memoir which I have not read, to have mathematically demonstrated that the gold of a crimson colour on porcelain is in the metallic state.

The purple tincture of gold might, perhaps, be made use of to advantage in the dyeing of silks, although the price might be high; the colour obtained from it surpasses every other in solidity, because there is no combustion which can destroy it. It will be prudent, however, to agitate the stuffs a long time in the tincture; and, to obtain shades more or less deep, it will be necessary to repeat the immersion of the stuffs more or less often, taking care to squeeze them well, and drying and shaking them between every immersion.

The gradation of shades through which a mixture of the nitro-muriatic and muriatic solutions of tin passes, weakened by pouring into it, drop by drop, a solution of gold with a great excess of acid, and very much diluted with water, indicates, as I think, a gradual oxidation. An acetic solution of iron, even, appears to ascertain the same; because, from a watery green, it acquires more and more a reddish yellow cast when exposed to the atmospheric air, or in contact with oxygen gas.

I have made it appear, in a memoir upon the tincture of the alkaline mars of Stahl, that the sulphate of iron may also hyper-oxygenate itself, and lose the excess of oxygen by the action of light. In making a mixture of the concentrated sulphuric acid and the nitric solution of iron, I obtained, after the evaporation of the nitric acid, and upon making the residue attract the humidity of the air, by allowing it to rest a few months, crystals of hyper-oxygenated sulphate of iron, which I could scarcely distinguish, owing to their whiteness, from sulphate of alumine; but the action of light gradually yellowed the surface: their whiteness could be restored, however, by a slight washing. We may in the same manner procure a sulphate of iron hyper-oxygenated, a little similar in whiteness, by precipitating the ni-

trate of iron, and dissolving this precipitate (sweetened, and freed from the greater part of the water), by little and little, with sulphuric acid, which ought to be concentrated, in order to obtain, without evaporation, crystals of sulphate of hyper-oxygenated iron. This salt possesses an incomparable astringency.

The operation of the transmission of oxygen appears to manifest itself the more upon pieces of linen simply dipped in acetate of iron, and maddered, since we are obliged, in order to bleach them completely, to expose them a long time in the field, if we do not choose to make use of the artificial process of bleaching. The printed parts of these pieces of cloth are often weakened, and appear sometimes as if pierced with a sharp instrument, or burned with a concentrated acid: such a circumstance could not occur, except from the action of the oxygen of the coloured oxide of iron, oxygen being successively restored to this oxidable body by the atmospheric air.

Minerals do not present the only substances which oxidate gradually, and by intermediate terms.

Indigo furnishes a proof that vegetables (and I may add also that animals) will produce similar results; for a solution of any kind of indigo (I except, however, the sulphate of indigo) may, in de-oxidating or in recovering oxygen, pass through all the degrees of the shades of blueish green, even to that of a very yellow olive, and yet preserve the same quantity of indigo in solution. The beauty and solidity of the blues for dyeing, and of the blue for pencilling, depend much on the different degrees of oxidation.

XIV. *On the partial Fusion of Metals by the Electric Discharge. By a Correspondent.*

To Mr. Tilloch.

SIR,

I TAKE the liberty of sending you some further experiments and observations on the partial fusion of thin plates of metal, &c. an account of which you was so kind as to insert in your

your Magazine of March 1805, under the title of "A new Electrical Phænomenon, communicated by a Correspondent." In these experiments, which were chiefly made with a shilling, I could not reconcile the idea of a fusion taking place, the force employed being so very small: there is, however, no doubt but this is a fusion both on the part of the piece and the coating of the phial.

Being induced to make these experiments with a greater degree of accuracy and observation, I began with a piece of money, as stated in my former paper: it is there said, "I sometimes observed a small hole in the coating where the piece was taken from:" I find that this is made at every discharge of the jar, whether the shilling is interposed or not. On examining this hole (which is just perceptible to the naked eye) with a microscope, I observed the coating curiously melted, very minute globules of the melted metal being scattered around it in every direction. Sometimes, when the jar is charged very highly, and the piece interposed, several of these holes are made, in each of which there is the same appearance. A small white spot, encompassed with a brown ring, is likewise observed on the shilling, which bears evident marks of fusion, though in a less degree than the tinfoil of the jar.

On passing twelve successive discharges through a shilling, the surface became quite brown for about half an inch in length and two lines in breadth. This colour was not permanent, but easily rubbed off with the finger. The white spots, however, made at every discharge remained, and seemed to be indented on the surface. It is somewhat remarkable, that in all these experiments the effect on that side of the piece which was next the coating of the jar was scarcely visible, though the tinfoil was melted as before. This favours rather, I think, the idea of a single electric fluid. In copper, this effect seems to partake more of a calcination or oxidation; as the part where the discharge was made through was always white, though still encompassed with the brown ring.

From a number of trials it appears that this effect is scarcely, if at all, visible on thin plates of iron. Though I

have subjected them to a greater force of electricity, yet I could never discover on them any marks of fusion, or that the metal was stained brown, as in the other experiments.

It is very surprising that so small a quantity of accumulated electricity should operate so powerfully on a plate of metal, when it requires a very large battery to melt a small wire! Many electricians have turned their thoughts to the melting of wires, &c. by the electric battery, amongst whom I would notice Mr. A. Brooks, late of Norwich, who seems to have treated this subject with great perspicuity and exactness; but very few of them thought of making the discharge through *plates* of metal; which seems to open a wide field for observation and experiment.

The insertion of the above in your publication will greatly oblige, sir,

Your obedient humble servant,

C. R.

XV. *On the Reproduction of Buds. By* Thomas Andrew Knight, *Esq. F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.**

MY DEAR SIR,

Every tree in the ordinary course of its growth generates, in each season, those buds which expand in the succeeding spring; and the buds thus generated contain, in many instances, the whole of the leaves which appear in the following summer. But if these buds be destroyed during the winter or early part of the spring, other buds, in many species of trees, are generated, which in every respect perform the office of those which previously existed, except that they never afford fruit or blossoms. This reproduction of buds has not escaped the notice of naturalists; but it does not appear to have been ascertained by them from which, amongst the various substances of the tree, the buds derive their origin.

Du Hamel conceived that reproduced buds sprang from pre-organized germs: but the existence of such germs has

* From the *Transactions of the Royal Society* for 1805.

not,

not, in any instance, been proved; and it is well known that the roots, and trunk, and branches, of many species of trees will, under proper management, afford buds from every part of their surfaces; and therefore, if this hypothesis be well founded, many millions of such germs must be annually generated in every large tree, not one of which, in the ordinary course of nature, will come into action: and as nature, amidst all its exuberance, does not abound in useless productions, the opinions of this illustrious physiologist are, in this case, probably erroneous.

Other naturalists have supposed the buds, when reproduced, to spring from the plexus of vessels which constitutes the internal bark; and this opinion is, I believe, much entertained by modern botanists: it nevertheless appears to be unfounded, as the facts I shall proceed to state will evince.

If the fruit-stalks of the sea cale (*crambe maritima*) be cut off near the ground in the spring, the medullary substance, within that part of the stalk which remains attached to the root, decays; and a cup is thus formed, in which water collects in the succeeding winter. The sides of this cup consist of a woody substance, which in its texture and office, and mode of generation, agrees perfectly with the alburnum of trees; and I conceive it to be as perfect alburnum as the white wood of the oak or elm: and from the interior part of this substance, within the cup, I have frequently observed new buds to be generated in the ensuing spring. It is sufficiently obvious that the buds in this case do not spring from the bark; but it is not equally evident that they might not have sprung from some remains of the medulla.

In the autumn of 1802 I discovered that the potatoe possessed a similar power of reproducing its buds. Some plants of this species had been set, rather late in the preceding spring, in very dry ground, where, through want of moisture, they vegetated very feebly; and the portions of the old roots remained sound and entire till the succeeding autumn. Being then moistened by rain, many small tubers were generated on the surfaces made by the knife in dividing the roots into cuttings; and the buds of these, in many instances,

elongated

elongated into runners which gave existence to other tubers, some of which I had the pleasure to send to you.

I have in a former paper remarked, that the potatoe consists of four distinct substances, the epidermis, the true skin, the bark, and its internal substance, which from its mode of formation and subsequent office I have supposed to be alburnous: there is also in the young tubes a transparent line through the centre, which is probably its medulla. The buds and runners sprang from the substance which I conceive to be the alburnum of the root, and neither from the central part of it, nor from the surface in contact with the bark. It must, however, be admitted, that the internal substance of the potatoe corresponds more nearly with our ideas of a medullary than of an alburnous substance, and therefore this, with the preceding facts, is adduced to prove only that the reproduced buds of these plants are not generated by the cortical substance of the root: and I shall proceed to relate some experiments on the apple, and pear, and plum tree, which I conceive to prove that the reproduced buds of those plants do not spring from the medulla.

Having raised from seeds a very considerable number of plants of each of these species in 1802, I partly disengaged them from the soil in the autumn, by digging round each plant, which was then raised about two inches above its former level. A part of the mould was then removed, and the plants were cut off about an inch below the points where the seed-leaves formerly grew; and a portion of the root, about an inch long, without any bud upon it, remained exposed to the air and light. In the beginning of April I observed many small elevated points on the bark of these roots, and, removing the whole of the cortical substance, I found that the elevations were occasioned by small protuberances on the surface of the alburnum. As the spring advanced, many minute red points appeared to perforate the bark: these soon assumed the character of buds, and produced shoots in every respect similar to those which would have sprung from the organized buds of the preceding year. Whether the buds thus reproduced derived any portion of their

their component parts from the bark or not, I shall not venture to decide; but I am much disposed to believe that, like those of the potatoe, they sprang from the alburnous substance solely.

The space, however, in the annual root, between the medulla and the bark is very small; and therefore it may be contended that the buds in these instances may have originated from the medulla. I therefore thought it necessary to repeat similar experiments on the roots and trunks of old trees, and by these the buds were reproduced precisely in the same manner as the annual roots: and therefore, conceiving myself to have proved in a former memoir* that the substance which has been called the medullary process does not originate from the medulla, I must conclude that reproduced buds do not spring from that substance.

I have remarked in a paper which you did me the honour to lay before the Royal Society in the commencement of the present year, that the alburnous tubes at their termination upwards invariably join the central vessels, and that these vessels, which appear to derive their origin from the alburnous tubes, convey nutriment, and probably give existence to new buds and leaves. It is also evident, from the facility with which the rising sap is transferred from one side of a wounded tree to the other, that the alburnous tubes possess lateral, as well as terminal, orifices: and it does not appear improbable, that the lateral as well as the terminal orifices of the alburnous tubes may possess the power to generate central vessels; which vessels evidently feed, if they do not give existence to, the reproduced buds and leaves. And therefore, as the preceding experiments appear to prove that the buds neither spring from the medulla nor the bark, I am much inclined to believe that they are generated by central vessels which spring from the lateral orifices of the alburnous tubes. The practicability of propagating some plants from their leaves may seem to stand in opposition to this hypothesis; but the central vessel is always a component part of the leaf, and from it the bud and young plant probably originate.

* Phil. Trans. of 1803.

I expected

I expected to discover in seeds a similar power to regenerate their buds; for the cotyledons of these, though dissimilar in organization, execute the office of the alburnum, and contain a similar reservoir of nutriment, and at once supply the place of the alburnum and the leaf. But no experiments which I have yet been able to make, have been decisive, owing to the difficulty of ascertaining the number of buds previously existing within the seed. Few if any seeds, I have reason to believe, contain less than three buds, one only of which, except in cases of accident, germinates; and some seeds appear to contain a much greater number. The seed of the peach appears to be provided with ten or twelve leaves, each of which probably covers the rudiment of a bud, and the seeds, like the buds of the horse-chestnut, contain all the leaves and apparently all the buds of the succeeding year: and I have never been able to satisfy myself that all the buds were eradicated without having destroyed the base of the plumule, in which the power of reproducing buds probably resides, if such power exists.

Nature appears to have denied to annual and biennial plants (at least to those which have been the subjects of my experiments) the power which it has given to perennial plants to reproduce their buds; but nevertheless some biennials possess, under peculiar circumstances, a very singular resource, when all their buds have been destroyed. A turnip, bred between the English and Swedish variety, from which I had cut off the greater part of its fruit-stalks, and of which all the buds had been destroyed, remained some weeks in an apparently dormant state; after which the first seed in each pod germinated, and bursting the seed-vessel, seemed to execute the office of a bud and leaves to the parent plant, during the short remaining term of its existence, when its preternatural foliage perished with it. Whether this property be possessed by other biennial plants in common with the turnip or not, I am not at present in possession of facts to decide, not having made precisely the same experiment on any other plant.

I will take this opportunity to correct an inference that I have

have drawn in a former paper*, which the facts (though quite correctly stated) do not, on subsequent repetition of the experiment, appear to justify. I have stated, that when a perpendicular shoot of the vine was inverted to a depending position, and a portion of its bark between two circular incisions round the stem removed, much more new wood was generated on the lower lip of the wound, become uppermost by the inverted position of the branch, than on the opposite lip, which would not have happened had the branch continued to grow erect; and I have inferred that this effect was produced by sap which had descended by gravitation from the leaves above. But the branch was, as I have there stated, employed as a layer, and the matter which would have accumulated on the opposite lip of the wound had been employed in the formation of roots, a circumstance which at that time escaped my attention. The effects of gravitation on the motion of the descending sap, and consequent growth of plants, are, I am well satisfied, from a great variety of experiments, very great; but it will be very difficult to discover any method by which the extent of its operation can be accurately ascertained, For the vessels which convey and impel† the true sap, or fluid from which the new wood appears to be generated, pass immediately from the leaf-stalk towards the root; and though the motion of this fluid may be impeded by gravitation, and it be even again returned into the leaf, no portion of it, unless it had been extravasated, could have descended to the part from which the bark was taken off in the experiment I have described. I am not sensible that in the different papers which I have had the honour to address to you, I have drawn any other inference which the facts, on repetition of the experiments, do not appear capable of supporting.

I am, &c.

THOMAS ANDREW KNIGHT.

Elton,
May 12, 1805.

* Phil. Trans. of 1803.

† Phil. Trans. of 1804.

XVI. *Pro-*

XVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

Feb. 6. The Right Honourable Sir Joseph Banks, Bart. President, in the chair. The reading of Mr. Hatchett's interesting paper on artificial tannin was resumed. *Effects of sulphuric acid on vegetables in converting them into coal.* Resins distilled yield but from ·03 to ·05 grains coal; but when treated with this acid from ·30 to ·60; with muriatic acid somewhat less. Gums, whether by distillation or digestion with acid, yielded nearly equal quantities of carbon. The author, from his numerous and very decisive experiments, proceeded with great caution to offer a theory of the formation of pit-coal; and his own discoveries have enabled him to adduce facts that seem to leave us without hope of ever attaining much more complete knowledge of the origin and formation of pit-coal than what he has modestly proposed. That all carbonaceous matter is of vegetable origin, has been generally allowed; and Mr. Hatchett thinks that the means employed by nature for converting vegetables into carbon has been by slow digestion of great masses exposed to the action of sulphuric acid. To this end he admits that muriatic acid may also have contributed; but from the much greater universality of sulphuric acid, sulphate of iron (pyrites), &c. in coals, and his synthetical experiments, which often gave 60 per cent. of carbon from vegetable matter by means of this acid, he thence concludes that sulphuric acid must have been the more efficient if not the sole agent. Carbon thus formed is found in many parts without bitumen, such as in Kilkenny and Bovey coals, &c. The bitumen is also formed of the resinous parts of vegetables subjected to the slow action of sulphuric acid.

Feb. 13. Mr. Hatchett admits that his efforts to form bitumen have not been completely successful, although there can be no doubt of its constituent principles. All synthetic experiments, he observes, are necessarily conducted too rapidly, and on too small quantities, ever to imitate effectually the process of nature. To the submarine composts or

vegetable solutions of certain philosophers he seems decidedly averse, and appears also willing to deny the agency of muriatic acid in the formation of coal. For this, however, he has not assigned any reason, although the almost invariable contiguity of lime to coal would tend to prove, that had any quantity of muriatic acid been present at the formation of either, a new and very different substance must have been produced. The few saline springs found in the vicinity of coal-mines, he considers as no exception to the general principle of the inefficiency of muriatic acid; and the discoveries of Mr. Peel may perhaps render the opinion of our author still more probable. Mr. Hatchett allows that animal matter may have contributed to the formation of pit-coal; but denies that any fire could have been accessory. The great frequency, indeed, of numerous animal substances found in the limestone that is contiguous to coal-strata, renders it difficult to deny that animal matter could not have been present at the original formation of coal. Mr. Hatchett concludes this very able paper with declaring the necessity of adhering invariably to that rigid systematic order which we hinted at in a former report, and without which, he observes, that science can neither be extended nor applied to the purposes of civil society. He also recommends such researches to the attention of other chemists, as he has determined to decline them for the present, and hopes that his discoveries and numerous experiments may be usefully applied to the arts, and the important œconomy of fuel, &c. The thanks of the Society were ordered to this philosopher, and we doubt not that he will also receive those of the civilized world.

On the same evening a letter from Mr. Griffiths to the President was read, containing a brief account of a species of worm-shell found by him in a bank of clay on the coast of Sumatra, after the shock of an earthquake. Considerable numbers of the same species are found in the surrounding seas, in water from one to six fathoms deep, and vary in length from three to five feet, and in diameter from three to nine inches. One of the specimens procured by Mr. Griffiths measures above five feet, is taper, has two tentaculi, and

and in other respects is somewhat different from the figure given by Rhumphius. Not one of those shells could be procured perfect in itself, but their figure may be correctly conceived from the different broken specimens. The body of the fish consisted of a gelatinous mass. The outside of the shell was white, the inside yellow, and its fracture strongly resembled stalactite.

Feb. 20. The introduction to a mathematical paper on "*Impossible* Quantities" was read, but it is impossible that we can report it.

A physiological paper, on a particular affection of the prostate gland, was also announced, the reading of which was deferred till next meeting.

BRITISH INSTITUTION FOR PROMOTING THE FINE ARTS.

The gallery belonging to this laudable institution was opened on the 17th of the present month (February) for the exhibition and sale of the productions of the British artists; and will continue open every day from 10 in the morning till 5 in the afternoon. It is but justice to say, that the display of taste, talent, and genius, which even the first opening has exhibited, are highly creditable to the British school. Many of the pictures possess the highest excellence, and all of them do honour to the individuals who have produced them.

SOCIETY OF ANTIQUARIES.

Feb. 6. Mr. Orde in the chair. Several curious historical notices were read on the character and office of the antient minstrels, or, as they were sometimes called, cytharists, in this country; from which it appeared that several females had assumed the character of wandering minstrels and dancers. Their loose manner of life soon rendered them obnoxious to the laws, and they were at length suppressed as common rogues and vagrants; but their music and dancing, we are happy to find, at no period, had any relation to the general licentiousness of persons of the like profession in every country on the continent.

Feb. 13. The right honourable president, Earl of Leicester in the chair. Mr. Lysons furnished some extracts from

the antient records of falconry, in the reign of Edward VI. The word *mews*, now improperly but generally applied to a pile of stables, was originally applied to a long range of buildings appropriated to the *mewing* and breeding of falcons.

An old painting, dated 1560, with the poetic inscription—"Rather deathe—Then false of faythe," was exhibited by a baronet, under the impression of its being a portrait of lady Jane Grey; but which might be more probably her mother, as supposed by the secretary, the Rev. Mr. Brand. This circumstance should teach amateurs not to be so credulous in purchasing old paintings.

ROYAL JENNERIAN SOCIETY.

At a special meeting of the board of directors, lately held at the central house of the society, No. 14, Salisbury-square, Fleet-street, the following report of the medical council, on the subject of vaccine inoculation, was laid before the board.

REPORT.

The medical council of the Royal Jennerian Society, having been informed that various cases had occurred, which excited prejudices against vaccine inoculation, and tended to check the progress of that important discovery in this kingdom, appointed a committee of twenty-five of their members to inquire, not only into the nature and truth of such cases, but also into the evidence respecting instances of small-pox, alleged to have occurred twice in the same person.

In consequence of this reference, the committee made diligent inquiry into the history of a number of cases, in which it was supposed that vaccination had failed to prevent the small-pox, and also of such cases of small-pox as were stated to have happened subsequently to the natural or inoculated small-pox.

In the course of their examination the committee learned, that opinions and assertions had been advanced and circulated, which charged the cow-pox with rendering patients liable to particular diseases, frightful in their appearance, and hitherto unknown; and judging such opinions to be connected

connected with the question as to the efficacy of the practice, they thought it incumbent upon them to examine also into the validity of these injurious statements respecting vaccination.

After a very minute investigation of these subjects, the result of their inquiries has been submitted to the medical council; and from the report of the committee it appears:

I. That most of the cases, which have been brought forward as instances of the failure of vaccination to prevent the small-pox, and which have been the subjects of public attention and conversation, are either wholly unfounded or grossly misrepresented.

II. That some of the cases are now allowed, by the very persons who first related them, to have been erroneously stated.

III. That the statements of such of those cases as are published, have, for the most part, been carefully investigated, ably discussed, and fully refuted, by different writers on the subject.

IV. That notwithstanding the most incontestable proofs of such misrepresentations, a few medical men have persisted in repeatedly bringing the same unfounded and refuted reports and misrepresentations before the public; thus perversely and disingenuously labouring to excite prejudices against vaccination.

V. That in some printed accounts adverse to vaccination, in which the writers had no authenticated facts to support the opinions they advanced, nor any reasonable arguments to maintain them, the subject has been treated with indecent and disgusting levity; as if the good or evil of society were fit objects for sarcasm and ridicule.

VI. That when the practice of vaccination was first introduced and recommended by Dr. Jenner, many persons, who had never seen the effects of the vaccine fluid on the human system, who were almost wholly unacquainted with the history of vaccination, the characteristic marks of the genuine vesicle, and the cautions necessary to be observed in the management of it, and were therefore incompetent to

decide whether patients were properly vaccinated or not, nevertheless ventured to inoculate for the cow-pox.

VII. That many persons have been declared duly vaccinated, when the operation was performed in a very negligent and unskilful manner, and when the inoculator did not afterwards see the patients, and therefore could not ascertain whether infection had taken place or not; and that to this cause are certainly to be attributed many of the cases adduced in proof of the inefficacy of cow-pox.

VIII. That some cases have been brought before the committee, on which they could form no decisive opinion, from the want of necessary information as to the regularity of the preceding vaccination, or the reality of the subsequent appearance of the small-pox.

IX. That it is admitted by the committee, that a few cases have been brought before them, of persons having the small-pox, who had apparently passed through the cow-pox in a regular way.

X. That cases, supported by evidence equally strong, have been also brought before them, of persons who, after having once regularly passed through the small-pox, either by inoculation or natural infection, have had that disease a second time.

XI. That in many cases in which the small-pox has occurred a second time, after inoculation or the natural disease, such recurrence has been particularly severe, and often fatal; whereas, when it has appeared to occur after vaccination, the disease has generally been so mild, as to lose some of its characteristic marks, and even sometimes to render its existence doubtful.

XII. That it is a fact well ascertained, that, in some particular states of certain constitutions, whether vaccine or variolous matter be employed, a local disease only will be excited by inoculation, the constitution remaining unaffected; yet that matter taken from such local vaccine or variolous pustule is capable of producing a general and perfect disease.

XIII. That if a person, bearing the strongest and most indubitable

indubitable marks of having had the small-pox, be repeatedly inoculated for that disease, a pustule may be produced, the matter of which will communicate the disease to those who have not been previously infected.

XIV. That although it is difficult to determine precisely the number of exceptions to the practice, the medical council are fully convinced that the failure of vaccination, as a preventive of the small-pox, is a very rare occurrence.

XV. That of the immense number who have been vaccinated in the army and navy, in different parts of the united kingdom, and in every quarter of the globe, scarcely any instances of such failure have been reported to the committee but those which are said to have occurred in the metropolis or its vicinity.

XVI. That the medical council are fully assured, that in very many places in which the small-pox raged with great violence, the disease has been speedily and effectually arrested in its progress, and in some populous cities wholly exterminated, by the practice of vaccination.

XVII. That the practice of inoculation for the small-pox, on its first introduction into this country, was opposed and very much retarded, in consequence of misrepresentations and arguments drawn from assumed facts, and of miscarriages arising from the want of correct information, similar to those now brought forward against vaccination, so that nearly fifty years elapsed before small-pox inoculation was fully established.

XVIII. That by a reference to the bills of mortality, it will appear that, to the unfortunate neglect of vaccination, and to the prejudices raised against it, we may in a great measure attribute the loss of nearly two thousand lives by the small-pox, in this metropolis alone, within the present year.

XIX. That the few instances of failure, either in the inoculation of the cow-pox, or of the small-pox, ought not to be considered as objections to either practice, but merely as deviations from the ordinary course of nature.

XX. That if a comparison be made between the preservative effects of vaccination, and those of inoculation for the

small-pox, it would be necessary to take into account the greater number of persons who have been vaccinated within a given time: as it is probable that, within the last seven years, nearly as many persons have been inoculated for the cow-pox as were ever inoculated for the small-pox since the practice was introduced into this kingdom.

XXI. That from all the facts which they have been able to collect, it appears to the medical council, that the cow-pox is generally mild and harmless in its effects; and that the few cases which have been alleged against this opinion may be fairly attributed to peculiarities of constitution.

XXII. That many well-known cutaneous diseases, and some scrophulous complaints, have been represented as the effects of vaccine inoculation, when in fact they originated from other causes, and in many instances occurred long after vaccination; and that such diseases are infinitely less frequent after vaccination, than after either the natural or inoculated small-pox.

Having stated these facts, and made these observations, the medical council cannot conclude their report upon a subject so highly important and interesting to all classes of the community, without making this solemn declaration:

That, in their opinion, founded on their own individual experience, and the information which they have been able to collect from that of others, mankind have already derived great and incalculable benefit from the discovery of vaccination; and that it is their full belief, that the sanguine expectations of advantage and security which have been formed from the inoculation of the cow-pox, will be ultimately and completely fulfilled.

(Signed)

Ed. Jenner, M.D. president.	W. Blair.
J. C. Lettsom, M. D. V. P.	Gil. Blane, M. D.
John Ring, V. P.	Isaac Buxton, M. D.
Joseph Adams, M. D.	Wm. Chamberlaine.
John Addington.	John Clarke, M. D.
C. R. Aikin.	Astley Cooper.
Wm. Babington, M. D.	Wm. Daniell Cordell.
M. Baillie, M. D.	Richard Croft, M. D.

Tho.

Tho. Denman, M. D.
John Dimsdale.
Henry Field.
Edward Ford.
Joseph Fox.
Will. M. Fraser, M. D.
William Gaitskell.
Wm. Hamilton, M. D.
John Hingeston.
Everard Home.
Robert Hooper, M. D.
Joseph Hurlock.
John Jones.
Thomas Key.
Francis Knight.
E. Leese.
L. Leese.
William Lewis.
William Lister, M. D.
Alex. Marcet, M. D.
Joseph Hart Myers, M. D.
James Parkinson.
Thomas Paytherus.
John Pearson.
George Rees, M. D.
John Gibbs Ridout.
J. Squire, M. D.
James Upton.
J. Christian Wachsell.
Thomas Walshman, M. D.
Robert Willan, M. D.
Allen Williams.
James Wilson.
J. Yelloly, M. D.

January 2, 1806.

John Walker,
Secretary to the council.

ACADEMY OF USEFUL SCIENCES AT ERFURT.

Professor Bernardi is returned to Erfurt from his five months tour through the Carinthian Alps. Notwithstanding the difficulties he had to encounter from unfavourable weather and the operations of the war, he has no reasons to be dissatisfied with the result of his journey. He has made a number of new discoveries which he is about to publish. In an essay read before the academy, on the 2d of December, he says: "It surely cannot be supposed that our country has been yet sufficiently examined. In the southern part of it, in particular, there are many hidden treasures of science; and it is incontestably a great error in our countrymen to travel into foreign countries, forgetting the treasures of which Germany has to boast." The first proof which the professor exhibited of this fact was by the communication of two kinds of the herb Speedwell or Fluellin, not yet hitherto sufficiently well described, and which nearly resemble the *Veronica spicata*. The one he calls *Veronica*

cristata, and the other *Veronica sternbergiana*. The first, which he found in Hungary near the German frontiers, and on the mountains of Dornach not far from Vienna, differs very little from the *Veronica spicata* in the form of the stalk and leaves, but much more in the blossoms and the stalk of the flower. It carries its blossoms in a proper ear. The folds of the flowers, which resemble a short pipe, are not so broad; they are rolled together to the very middle, and do not present a genuine pipe; so that the flower assumes a funnel-like appearance, and approaches very nearly in its stalk to that of the *Veronica sibirica* and *virginica*. In respect to other particulars it does not differ very much from the *Veronica spicata*, which is already accurately described. In the botanical system it has the following characteristic—Ver. spica terminali, corollæ subrotatæ, laciniis postice convolutis, falcis oppositis.—The second specimen Count Sternberg already remarked in Italy in the *Sette communi*, and gave it the name of *Ver. glabra*. But as Ehrhardt had made use of this name to another kind of Veronica, Bernardi thought proper to name it after its discoverer. It principally grows at Krain, near Jauernberg, on the hills which surround the foot of Belsbiza. Its stalks and leaves are almost entirely hairless; the first is very lank, and its longer-stalked blossoms stand somewhat crowded together; it may be recognized by the following difference—V. racemo terminali, corollæ rotatæ, laciniis patentibus, foliis oppositis, caulcque glabris.—Professor Bernardi has more clearly described the doubtful Speedwells or Fluellins—V. urticæfolia, latifolia, Teucrium, prostrata et pilosa, and has assigned to them proper descriptions. This essay of Professor Bernardi will appear in the acts of the academy, a new volume of which will be immediately published.

ACADEMY OF SCIENCES, GOTTINGEN.

This academy has added the names of Chaptal and Cuvier, both members of the National Institute of France, to the list of their foreign associates.

XVII. *Intel-*

XVII. *Intelligence and Miscellaneous Articles.*

PRODUCTION OF MURIATES BY THE GALVANIC DECOMPOSITION OF WATER.

To Mr. Tilloch.

SIR, Leicester, Feb. 15, 1806.

I HAVE the pleasure to inform you I have tried Mr. Peel's experiments relative to the formation of muriat of soda and muriat of potash, which were in your Magazine in April and July last, and find them to be perfectly correct.

Yours truly, &c. &c.

A. B. HORTENTZ.

BEET-ROOT SUGAR.

A manufactory for this sugar has been established by a company of merchants in the neighbourhood of Moscow; and the success of the establishment has proved, that from this vegetable a sugar is obtained which not only finds a ready market, but which, the consumers say, yields in no quality whatever to the sugar generally in use.

A specimen has been presented to his majesty the emperor of Russia, who was pleased to express his high satisfaction with it.

CURE FOR DROPSY.

We have already had occasion to announce, that different people who had been afflicted with dropsy, had been perfectly cured by means of Bohea tea. In the Greenock Advertiser we find the following instance of the efficacy of this remedy:

A correspondent in Islay requests us to state, for the information of the public, that Donald Cameron, a herd upon the farm of Machry, in that island, was severely afflicted with a dropsy, which kept him bed-fast from July to November last; but was finally cured by using a quarter of a pound of Bohea, drinking a strong infusion of it, and eating the leaves. In three days the immense quantity of water that was in his body disappeared, the tea having acted as a powerful diuretic; and he is now stout and hearty,

after being despaired of and given over by all of the faculty that saw him.

CURE FOR DEAFNESS.

Petersburgh, 22d January.

Demetri Simeonovitsch Sitnikoff, a merchant in Moscow, was for half a year deprived of the faculty of hearing, and all the remedies usually prescribed for that disease were used without effect. At last he had recourse to the following simple operation: After having filled his mouth with the smoke of tobacco, he closed it firmly, and also his nose, thereby forcing the smoke to find its way through the ears. The following morning he felt a crash, first in one ear, and then in the other, and from that moment his hearing has been completely restored.—*Hamburgh Correspondenten, February* 8.

FOSSIL SKELETON.

At Sir Joseph Banks's conversationi on the 2d of February, ——. Hawker, esq. of Dudbridge, Gloucestershire, exhibited complete drawings, and several of the bones of a large fossile animal similar to a crocodile, found in a solid stratum of limestone 20 feet thick. It was imbedded 15 feet below the surface of the stratum. The skeleton measures 10½ feet in length, and all the parts are wonderfully perfect. The jaws which were exhibited contained the teeth in high preservation, and still covered with the enamel. One of them which was broken had so exactly the fracture of what is called petrified wood, that it would have deceived the most acute mineralogist, and furnishes a strong ground for suspicion that many fossils generally held to be of vegetable are of animal origin. In the same stratum of limestone are found many cornua ammoniæ, mussels, and other shells.

HOLBEIN'S DANCE OF DEATH.

It is well known that Basil in Switzerland was celebrated for Holbein's celebrated picture of the Dance of Death, which had undergone several repairs from time to time, and had been recently retouched, to the great satisfaction of all the connoisseurs. In the evening of the 6th of August last, however, a mob collected in Basil, and, accompanied by a great

great number of women carrying lanthorns, broke in upon the gallery in the church-yard, which contained this masterpiece of antiquity, tore it from the walls, and in a few minutes succeeded in completely destroying it!

FALLING BODIES.

Mr. Benzenberg, professor of physic and astronomy at Dusseldorf, has published an account of twenty-eight experiments made in the coal mines of Schebusch with balls well turned and polished. They were made to fall from a height of 262 French feet. At a medium they produced a deviation of five lines towards the east: the theory gives 4·6 lines. These experiments furnish an additional proof, if any were wanted, of the rotary motion of the earth. Experiments made at Bologna by Mr. Guglielmini gave nearly the same results.

PROGRESS OF LITERATURE IN RUSSIA.

In the year 1804, fifteen new journals were printed in Russia, and 115 new books were published the same year at St. Petersburg and Moscow: among the latter were translations into the Russian language of the following works—Sterne's Tristram Shandy; Rousseau's Confessions, and his Eloisa; Hufeland's Art of prolonging Human Life; Barthelemy's Travels of Anacharsis; besides a variety of original works in the Russian language.

RUSSIAN EMBASSIES TO PEKIN AND JAPAN.

Petersburgh, Jan. 13.

Several persons who were appointed to travel with the embassy to Pekin have returned from Irkuzk, and have already arrived here. The counsellor of state, Fosse, who accompanied the chamberlain Resanow on the embassy to Japan, has also arrived here from Kamtschatka. After a stay of seven months, the embassy left Japan and returned to Kamtschatka in the Naveschda, a vessel commanded by captain Krusenstiern. Chamberlain Resanow set out from thence to Kodiak, and the other possessions of the Russian American company, according to his instructions, after having dispatched counsellor Fosse here by the way of Ochozk. Captain Krusensteirn sailed with the rest of the embassy for Canton,

Canton, their original destination, whence, it is expected, they will return next August. As far as we learn, no accident had befallen the embassy, all the members of which were in good health. The Naveschda left Kamtschatka in the middle of June.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Sampson, of Liverpool, in the county of Lancaster, wheelwright; for certain improvements in the application of power employed mechanically, especially as adapted to the use of cranks, and fly wheels or other contrivances producing equivalent or similar effect. Dated February 12, 1806.

To John Phillips, of East Stonehouse, in the county of Devon, stonemason and sculptor; for improvements in the construction of tinder-boxes. Dated February 12.

To John Phillips, of East Stonehouse, in the county of Devon, stonemason and sculptor; for a chain and apparatus for straight, square, and parallel stone and marble sawing; which chain may be applied to other useful purposes. Dated February 12.

To John Marshall, of Northwick, in the county palatine of Chester, salt proprietor; and John Naylor, of Hartford, in the same county, salt proprietor; for a new and improved method or manner of manufacturing and making salt. Dated February 14.

To Thomas Kentish, of Baker-street, North, in the parish of Saint Marylebone, in the county of Middlesex, esq.; for certain improvements in the construction of machines or engines applicable to the moving, raising, or lowering of heavy bodies and weights of all kinds, either upon land or on board of ships and vessels. Dated February 20.

To John Woodhouse, of the parish of Heyford, in the county of Northampton, engineer; for certain improvements relative to canals. Dated February 20.

To John Jones the younger, of Birmingham, in the county of Warwick, tool-maker and die-sinker; for improvements in the mode of manufacturing barrels for fire-arms. Dated February 20.

METEORO-

METEOROLOGY.

Monthly recapitulation of meteorological observations made at the forest four miles east of the river Mississippi, in latitude 31° 28″ north, and in longitude 91° 30″ west of Greenwich, for the year 1800. By William Dunbar, esq.

Year 1800.	Thermometer.			Barometer.			Rain.
	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Total.
	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Inches.
January	68	21½	43½	30·05	29·60	29·90	1·32
February	72	25	42½	30·10	29·52	30·02	3·11
March	78	28	58½	30·00	29·38	29·78	1·53
April	85	44	66¼	30·07	29·65	29·84	6·92
May	90½	61	72	29·95	29·62	29·76	3·94
June	96	61	79½	29·99	29·61	29·80	1·26
July	96	63	81½	29·99	29·78	29·90	4·83
August	96	70	82	29·99	29·71	29·86	0·35
September	90	59	76½	29·99	29·71	29·83	4·40
October	87	36	66	30·29	29·78	30·19	0·40
November	79	22	56	30·43	29·91	30·13	0·03
December	75	12	47¼	30·30	29·74	30·05	3·00
Whole Year	96	12	64¼	30·48	29·38	29·92	31·09

Mr. Dunbar's general observations on each month say, The month of May is the most agreeably temperate, and one of the driest; but the present is an exception to the rule.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For February 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Jan. 27	33°	39°	34°	29·10	5°	Cloudy
28	34	36	34	·10	4	Cloudy
29	34	36	34	·15	0	Snow
30	28	34	33	·15	5	Cloudy
31	28	37	30	·54	10	Fair
Feb. 1	28	42	31	·84	16	Fair
2	31	37	32	·90	0	Snow
3	32	35	31	·62	7	Cloudy
4	33	37	35	·62	0	Cloudy
5	46	53	47	·64	18	Fair
6	41	47	46	·70	9	Showery
7	45	53	46	·60	24	Fair
8	44	52	46	·80	20	Fair
9	44	47	40	·59	0	Rain
10	40	48	39	·73	15	Fair
11	37	47	41	·82	10	Fair
12	37	44	39	·84	26	Fair
13	40	44	32	·69	15	Cloudy
14	33	41	36	·40	0	Rain
15	37	42	37	·56	0	Rain
16	38	44	37	·84	7	Cloudy
17	36	47	35	30·00	21	Fair
18	35	42	31	·20	15	Cloudy
19	38	43	39	·23	15	Cloudy
20	39	43	40	29·95	10	Cloudy
21	41	50	45	·83	7	Cloudy
22	46	52	40	·61	10	Fair
23	42	50	44	·80	12	Cloudy
24	44	55	48	30·20	22	Fair
25	46	54	47	·22	15	Cloudy

N. B. The barometer's height is taken at noon.

XVIII. *On the Possibility of naturalizing the Cachemire Goat of India to the Climate of Europe: in a Letter from Dr.* De Carro, *of Vienna, to Professor* Pictet, *of Geneva*.*

Vienna, July 8, 1805.

You remember, sir, undoubtedly, the project which I communicated of profiting by the complaisance of numerous correspondents whom I have in different parts of Asia, and of the obligations which they acknowledge they owe to me for having furnished them with the means of propagating vaccination through a great part of the East. You know that my principal aim was to direct their attention to the alimentary plants of Asia, the culture of which might be useful in Europe. Of such plants I have continually solicited the seeds, with instructions for the necessary culture, relying at all times in this search on the information of the learned Dr. Roxburgh, manager of the botanic garden at Calcutta. I have also had recourse to you, sir, in requesting your suggestions on those objects which might be particularly useful to our country. You have thought of mountain rice. I have asked it from my correspondents; and, in spite of the small hopes of success which your experiments have hitherto afforded me, I shall still submit to your care and experiments the specimens of such articles as I receive from the East. You have, among other things, enjoined me to consider if means could be used to bring to Constantinople a pair of those famous goats of which the wool is manufactured into those beautiful and costly Cachemire shawls, so much in fashion throughout Europe and Asia.

Considering the difficulties of such an enterprise, it is proper to cite, perhaps, the very passage of your letter of the 21st of March 1805:—"Do you believe it possible to bring to Constantinople a pair of shawl goats? The English have not succeeded in naturalizing this Indian animal to their own climate. A single goat was brought to England,

* From *Bibliotheque Britannique*, vol. xxix.

but died almost immediately on its arrival. The attempt, however, may have been made without suitable care. My experience in sheep makes me unwilling to believe in the impossibility of naturalizing a breed to any climate. But it is an enterprise for a prince: for the shawl goat must be brought from the mountains of Thibet, and there would be many difficulties to surmount in obtaining a specimen. It is a problem which I submit to you for solution." Judging, as you do, that the difficulty of bringing goats from Thibet would be in fact insurmountable, I set about obtaining information if there was no climate nearer Constantinople to which this animal was naturalized. This was my object in a note which I sent on the 4th of April last to Mr. Wallenbourgh, who passed many years at Constantinople when employed in the Imperial legation, but now in the state chancery at Vienna, and who is well skilled in the languages and commercial affairs of the East. He had the goodness to communicate to me the answer which he received, and to allow me to make what use I might please of it.

"*Mr. Cogiasar de Sophialy to Mr. Wallenbourgh.*

"Although during my stay of some years at Bagdad I busied myself in collecting all possible information concerning the manufacture of Cachemire shawls, yet I was desirous, if possible, to obtain some further information from a Persian merchant lately arrived here, that I might answer the note I received from you. The animals whose fleece affords the Cachemire shawls are not goats, but sheep. There is a distance of 305 conaks (days' journeys) between the mountains where they are found, and Bagdad. To be sure of preserving a couple of pairs of those sheep, it would be necessary to make a purchase of twenty pairs for the journey, as the change of climate, and the length of the way, would be very dangerous to them. It would be necessary to send to a confidential person to make this purchase, and to conduct the animals hither; and he could scarcely return in less than five years, owing to the mode of travelling. It is well known that these journeys are made with caravans of camels;

mels; and the same camels that leave this place do not go on to Bagdad, these caravans being bound only from one town to another; so that he is often obliged to wait on his journey till another caravan is ready to set off. The khans or chieftains of these countries are also frequently at war with each other; and the traveller may be sometimes shut up six months, or a year, in some town, from which he dares not attempt to stir.—Such are the reasons of the journey being so tedious.

"It is impossible to bring the sheep in question in any other way than by these caravans of camels, by putting a pair in panniers upon each camel. This would cost at least 1500 piastres for the hire of the camels; besides, at least, 1500 more, to pay the confidential person employed to purchase and convey them: and besides all this, it would be still necessary to hire three or four persons to take charge of those animals during a long journey.

"It is questionable, however, how far one could count on the fidelity of this description of people; for they are often tempted by the love of gain, instead of fulfilling their commission, to stop in those distant countries, where one can only hear of them once a year, and speculate with the money which has been advanced to them. We have experienced this ourselves. Twelve years ago we intrusted 50,000 piastres to a factor who went to get shawls manufactured for us at Cachemire; and we are still waiting his return: in the mean time he is amusing himself with speculations at our expense.

"Thus, setting aside these inconveniences, it would be necessary to calculate on an expense of more than 50,000 piastres, without the certainty, even if the object were attained, of saving a couple of pairs of the sheep. Their food is herbage; but, as they have to pass through deserts, it would be necessary to have always sufficient quantities in store; and this would require still more camels for its carriage, which must, of course, augment the expense.

"If the person who desires to have two or three pairs of these animals, wishes them as curiosities, he must risk 50,000 piastres; but if it be for the purpose of multiplying

the breed in Europe, and obtaining their fleece for the manufacture of shawls like those of Cachemire, I doubt if he could succeed at all. The change of climate would, at first, assuredly deteriorate the fleece. Besides, I am certain that it would be impossible to imitate the shawls of Cachemire. Their goodness, their elasticity, the liveliness of their colours, are owing to the water of the river which passes through the city; and the same shawls, when washed in the same river fifteen miles further down, do not acquire the same good qualities: the difference is incredibly striking both to the feeling and sight. It would be therefore useless to bestow so much expense on this object.

"Such, sir, is the best information I can send you on the subject of your inquiries; I am sorry I can send you none more favourable. With respect, I am yours, &c.

"(Signed) COGIASAR DE SOPHIALY*."

All these details justify the opinion you have expressed of the great difficulty of the enterprise. It is not, however, such as a government, or a rich company, could not attempt. If such information could be turned to use, I should rejoice to have communicated it. You have stated, on the authority of some travellers, in some passages of the *Bibliotheque Britannique*, that the animal in question is a goat: according to the preceding letter it is a sheep. Which is in the right? If it be Mr. Cogiasar de Sophialy, it is for you, sir, a great encouragement to continue your efforts for the amelioration of wools, and the imitation of the Cachemire shawls.

Observations of M. Pictet on the preceding Letter.

It is in the valleys and on the mountains of Great Thibet that the animal lives which furnishes materials for the manufacture of Cachemire shawls. The distance is thirty days' journey eastward from Cachemire to the place where they are found. (See Forster's Travels in Cachemire.) We are entitled to call this animal by the name of goat, on the au-

* Mr. Cogiasar de Sophialy is, according to Mr. De Wallenbourgh, one of the greatest merchants of Constantinople, an Armenian by birth, and the most skilful person in commercial matters to whom he could have addressed himself.

thority

thority of a passage taken from Turner's Relation of an Embassy to Thibet, published in 1800. The English ambassador had passed the Bootan and approached *Teshoo Lamboo*, the residence of the lama, in the 28th degree of north latitude, before he saw any herds of those precious animals.

" The shawl goats feed in numerous herds on the short and scanty herbage which scarcely covers those elevated places. This animal is, perhaps, the most beautiful of all those that compose the numerous family of the goat kind. Their colour varies much. Some are black, some white, some blueish, and others of a tint a little lighter than that of a young fawn. This goat has straight horns, and is shorter in height than the smallest English sheep. The matter employed for the fabrication of shawls is a fine down which lies close to the skin. A long coarse hair covers this down, and serves to preserve its softness."

Forster, who carefully examined the valley of Cachemire, calls the matter of which these shawls are made wool. But he never saw the animal; and this expression no more proves its being the production of a sheep, than the term *vigonia* wool proves that the vigon is not of the goat kind. We know that the camel, the paca, the glama, the vigon, the goat of Tauris, and one of the varieties of the goats of Syria, yield, under the long and coarse hair, a down more or less fine. It is true that there are races of sheep, such as those of Shetland, those of Iceland, and some parts of Siberia and Tartary, which have in like manner a fine and soft down under their rough wool. Thus, the animal of Thibet which produces the precious matter of the shawls may, perhaps, be a sheep. By its dimensions it should belong rather to the sheep than the goat race, and it would not be astonishing that travellers should have made some mistake. Samuel Turner inspires confidence in his observations respecting natural history. He appears accurate in all the details relating to his mission. He describes distinctly all the animals of those high regions, and, in particular, the sheep of the country. The manner in which he speaks of the flocks of shawl goats leaves no room to doubt that he has examined

those animals minutely. The word *goat*, which the English, who are best acquainted with those countries, have given the animal, is a strong indication that it is not a sheep, since they would have called them shawl sheep if it had shown the characters belonging to that race.

The important point, however, is not its being a goat or a sheep, but whether it can be transported and naturalized to the climate of Europe. When we gave our correspondent, half in joke, half in earnest, the problem to solve, whether a pair of those goats could be brought to Constantinople, we thought of the possibility of bringing them from the mountains of Lesser Thibet, near the country of Cachemire, and where the analogy of climate seems to indicate that they could live equally well. From the mountains of Lesser Thibet the river Giluma runs into the Caspian sea. The navigation of this river from Badacian to Sellizure is 400 leagues; and the navigation of the Caspian, from Sellizure to Feorabath or to Terki, is about 200 leagues. From thence would remain the passage by land to Trebisond on the Black sea, or by the route to the mouth of this river, which, as is well known, formerly served for transporting merchandize from India.

The letter of our friend's correspondent gives, perhaps, an exaggerated idea of the real difficulties of transporting this animal: but the greatest difficulties might still occur in the proper regimen for their preservation in Europe. Thibet is the highest country of the old continent, and the air is uncommonly dry during the season which is not properly the rainy season, *i. e.* from October to June or July. The herbage of the pastures grows brown and dry, so as almost to crumble to powder when touched. It is this dried grass, very scanty in quantity, but apparently containing much esculent substance in small volume, which is the sole nourishment of the shawl goats. Could the animal subsist without a similar nourishment? Would it be possible to procure the same grass in some parts of the Pyrennees, or the Alps? Is it not necessary for the animal to have a far southern climate, and a soil extremely dry? If it could live in another climate, would it retain the down which preserves it from the

the cold? or would this down preserve its softness, its fineness, its strength and elasticity, which constitute its value?

As to the secret virtue of the river which runs by Cachemire, and which it loses further down, it may perhaps be of that kind of virtue which was attributed exclusively to the little river of the Gobelins. In all countries, the people who have brought any process of art to perfection are interested in throwing a mystery over the operations to which they owe their success. If this idea of obtaining the shawl goats to Europe be not chimerical, it is for France to realize it, since it possesses the greatest variety of climates.—Our readers will judge by this specimen of the benevolent activity of Dr. De Carro, whose success in propagating vaccination has only increased his zeal for the advancement of useful knowledge.

XIX. *An Essay on the Theory of mixed Gases, and the State of Water in the Atmosphere. By Mr.* John Gough *.

Four essays appear in the fifth volume of the Memoirs of the Literary and Philosophical Society of Manchester, which contain many new ideas relating to the constitution of mixed gases, and the state of water in the atmosphere. The design of these papers is evidently intended to remove certain difficulties which must strike every man of science who happens to peruse M. de Luc's theory of atmospherical vapour. This attempt has the double recommendation of ingenuity and novelty; but the leading opinions of the system, even in its present form, are liable to several objections, which I am going to point out, being generously invited to undertake the task by the author himself. My doubts relative to the subject arise partly from mathematical considerations, and in part from the evidence of experiment. Certain objections of the first class dispose me to conclude, that an atmosphere constructed on Mr. Dalton's plan will appear upon examination to be repugnant to the principles of the mechanical philosophy; and a direct appeal to experiment has moreover con-

* From *Manchester Transactions*, second series, vol. i.

vinced me that well established facts contradict the essential points of the theory.

To begin with the objections of the former class: I am ready to admit the existence of a fluid mixture, such as we find described at page 543, in the fifth volume of the Manchester Memoirs, with this reservation, that the concession is made merely for the purpose of showing such a combination to be incompatible with the usual course of things for a moment; which being demonstrated, the inutility of the fundamental hypothesis will follow, as a necessary consequence. To give a concise view of Mr. Dalton's general notion of the subject, we are to suppose a number of distinct gases to be confined in a space common to them all; which space may be circumscribed by the concave surface of a vessel, or the compressing power of an external fluid: besides this, we must imagine the constituent particles of each individual gas to be actuated by a mutual repulsion, while at the same time they remain perfectly indifferent to the particles which compose the other fluids that are confined in the common space: in short, we are to conceive that the particles of each gas act upon those of their own kind in the manner of elastic bodies, but that they obey the laws of inelastic bodies as often as they interfere with corpuscles of a different denomination. After premising the preceding particulars, we may conceive a certain arrangement of the elementary parts of a fluid mixture, in which the adjustment of the whole shall be of a description which will form, from particles of any one denomination, a homogeneous fluid possessing its own separate equilibrium: consequently each gas will exist as an independent being, and exercise the functions of its elasticity just as if all the other fluids were withdrawn from the common space. This systematic arrangement in an assemblage of gaseous substances cannot be maintained unless one particular method of disposing its component parts be observed, which consists in that distribution of the elements which will produce a separate equilibrium in the fluid composed by the elementary corpuscles of each denomination; consequently, the equilibrium in question cannot take place unless the necessary disposition

disposition of the heterogeneous particles be first established; so that the former requisite of the theory is entirely dependent on the latter. After having acquired a distinct idea of a fluid mixture, composed of gases possessing separate equilibria, we come in the next place to investigate the mechanical properties of such a compound; in the prosecution of which inquiry the comparative densities of the constituent fluids must be first determined in a horizontal plane, the situation of which is given in the common space.

Let the figure PMINKV (Plate I. fig. 2.*) represent this space, in which MVNK is the given plane. Now since every point of this plane may be supposed to be at an equal distance from the earth's centre, the density of every homogeneous gas supported by it will be the same in all parts of it. Let the constituent fluids be denominated A and B; also let C denote the compound; moreover let the densities of A and B, at P, be p and q; let PX and XY be two equal evanescent parts of the line PV. Now, seeing the pressure acting upon an elastic fluid is as the density of it, the fluxionary increments of p and q are as these quantities; but the densities of A and B, in the point X, are equal to the sums of p and q united to their increments respectively: let these sums be called e and f; then e is to f as p is to q, by composition of proportion: in like manner we find the density of A at Y to be to that of B at the same point as e is to f; *i. e.* as p is to q; thence it follows, that the fluxionary increments of the two densities have universally the given ratio of p to q; consequently the contemporary fluents, or the densities themselves, have the same given ratio: now what has been proved of the two gases A and B may be extended to any other number, viz. the ratios of their densities on the same horizontal plane will be given.

The ratio of A, B, &c. being found to be constant, we can proceed to investigate the proportions of the quantities of matter contained in these fluids. Let D and d be the densities of A and B in the plane MKNV; also let W and w be the quantities of matter of each kind contained in the variable space PMKNV; call PV x, and the area of the

* Given in our last number.

plane

plane MKNV y: now the fluxion of the space PMKNV is expressed by y into the fluxion of x; moreover the quantities of matter in two solids are in the complicate ratios of their magnitudes and densities, or in that of the densities only, if their magnitudes be equal; therefore the fluxion of W is to that of w as D is to d, because the fluxionary magnitude is common both to W and w; but D is to d as p to q, a constant ratio; consequently fluxion of W is to fluxion of w as p is to q; therefore W has to w the same given ratio; that is, the matter in A is to the matter in B as p is to q. In the next place let R and r be the distances of the centres of gravity of A and B, from the point P, taken in the line PI: then R into the fluxion of W is equal to the product of D, Y, x, and the fluxion of x, from a well known theorem in mechanics; for the same reason r into the fluxion of w is equal to the product of d, y, x, and fluxion x; hence R into fluxion of W is to r into fluxion of w as D is to d; but D is to d as fluxion of W is to fluxion w; therefore R and r are equal: consequently the centres of A and B coincide, and the point of their coincidence is also the centre of the system C. Thus it appears, that when the component gases of a fluid mixture possess separate equilibria, their densities are every where in a given ratio, and they have a common centre of gravity: the converse of which is equally true, viz. if their densities be not every where in a given ratio, and if they have not a common centre of gravity, they do not possess separate equilibria.

It is necessary to observe in this stage of the inquiry, that though we admit the particles of A and B to be inelastic in relation to each other, the concession must be strictly confined to the particles themselves; for the gases which are composed of them are elastic bodies; they therefore receive and communicate motion according to the laws which are peculiar to bodies of this description. The foregoing properties of a fluid mixture, which has been supposed to be duly adjusted, are now to be used in the examination of the fundamental proposition of the new theory intended to explain the constitution of the atmosphere. According to this proposition, if two gases come into contact the particles of which

which are perfectly inelastic in respect of each other, the particles of A meeting with no repulsion from those of B further than that repulsion, which as obstacles in the way they may exert, would instantly recede from each other as far as possible in their circumstances, and consequently arrange themselves just as in a void space. The preceding are the words of the author of the theory; and it is readily granted that the particles of such a heterogeneous mixture would recede from each other as far as circumstances will permit; the present subject of inquiry, then, brings the dispute to this issue—Can that arrangement take place amongst the particles of two or more gases, which will make their centres of gravity coincide in one point? For the separate equilibria of the fluids which enter into the constitution of the compound, will not be established until this arrangement be perfectly formed. The completion of this process being essential to the new theory, the effect of it has been, perhaps, too hastily inferred in the fourth proposition of Mr. Dalton's first essay; for I am sorry to observe that the inference is not supported by demonstration drawn from the doctrine of mechanics. It is the business of the present essay to supply what has been omitted, and to investigate the consequences which must arise from the collision of two heterogeneous gases differing in their specific gravities.

The existence of the fluid mixture, required by the theory, has been granted already for the sake of argument; and in order to continue the inquiry it must be remarked at present, that the necessary internal arrangement of the compound C, is liable to be disturbed perpetually by accidents resulting from the course of things; to which course the author of the theory undoubtedly wishes to accommodate his ideas. The preceding assertion may be exemplified in a manner which is familiar, and may be applied with ease to natural phænomena: let us suppose, then, an additional quantity of the gas A to be thrown into the pneumatic apparatus containing the compound C, which was in a state of proper adjustment previous to this event. No one will imagine that this fresh matter can diffuse itself through the mass of C with the same expedition that the electric fluid shows

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in expanding along a conductor: this supposition is contradicted by various appearances, from which the following one is selected,—agitation is known to accelerate the union of oxygen and nitrous gas. The quantity of A then, which has been newly admitted, will remain at first unmixed with B; but it will act immediately with a repulsive force upon kindred particles diffused through the compound C. This new modification of A will not preserve the density of its parts every where in a constant ratio to the density of the corresponding parts of B; and this change will disjoin the centres of gravity of A and B, which has been proved above. But when these points are placed apart, the separate equilibria of the fluids cease to exist, which has also been demonstrated before: therefore A and B begin to act and react mutually; which circumstance disturbs the necessary adjustment of C, and forces it to assume another character. It has also been proved in a former paragraph, that the two fluids will act upon each other in the manner of elastic bodies, even when the heterogeneous particles are supposed to be mutually inelastic; consequently A and B will begin to obey the law of their specific gravities as soon as their centres of gravity are separated by introducing into the space occupied by C, a fresh quantity of A or B: in consequence of this alteration the centre of gravity of the heavier fluid will begin to descend, while that of the lighter moves upwards. When once the centres of two gases are placed apart, their separation will become permanent; because, when at a distance, they are urged in opposite directions by a force resulting from the difference of the specific weights of the two fluids; and this contrariety of efforts must continue so long as the two centres are disjoined; consequently this opposition of force must be lasting, seeing nothing can put an end to it but an union, which it will always prevent. Nor can the mutual repulsion of the constituent particles of each gas, considered apart, in any manner promote the junction of the centres of gravity of the two fluids, because the action and re-action of a number of bodies amongst themselves do not alter the state of their common centre of gravity, whether it be at rest or in motion: so that A and B are under

the necessity of observing the law of their specific gravities, just as if the kindred particles of each fluid were actuated by no reciprocal repulsion nor any other cause of re-action. The doctrine of gases, which are mutually inelastic, is rendered indefensible by the preceding arguments; for the hypothesis is thereby exposed to a difficulty which, the author of the theory justly remarks, makes a mixture of mutually repulsive gases of different specific gravities an improbable conjecture; so that his own objection ultimately discountenances the leading opinions of that theory which it induced him to adopt in particular. At the same time philosophers are convinced that the atmosphere is a compound of gases possessing various degrees of specific weight; they moreover know that different chemical agents perpetually disturb the equilibrium of the compound, as some of them constantly absorb while others unfold the gases of which it is composed. The preceding facts are certain; consequently the heterogeneous elements of the atmosphere must be united by a common tie, which may be denominated a species of affinity, at least while our knowledge of the subject remains in its present imperfect state. The transparency of the great body of air surrounding the earth, also affords a strong argument for the chemical union of its component fluids, and at the same time discountenances the idea of the compound being a mechanical mixture of any description whatever; for when a number of diaphanous bodies of different specific gravities are mixed together, they form an aggregate which is opaque; but the union of the substances by fusion renders the mass transparent in many instances. Now, as the atmosphere is diaphanous, we are obliged, by the principles of sound argument, to consider it in the light of a compound, the ingredients of which are united by a chemical tie. Whatever may be the condition of the elastic fluids which enter into the composition of common air, one thing is certain, from a preceding paragraph of this essay, namely, no one of them can maintain a separate equilibrium as long as it makes an individual of the aggregate; consequently each particle of the compound must be urged by a force resulting

sulting from the general action of the mass, not by a pressure occasioned by a particular member of it.

On this account it is impossible for the aqueous part of common air to preserve the character of a gas at low temperatures, because steam cannot support 30 inches of mercury unless it is heated to 212 degrees of Fahrenheit's thermometer: were it then practicable to mix vapour of a less heat with atmospherical air, the spring of the gases would reduce it in an instant to the state of a liquid; so that the difficulty which renders De Luc's theory objectionable in its original form, is not removed in reality by the present modification of it.

The theory of mixed gases has been found to be indefensible on the principles of the mechanical philosophy; and I suspect that part of it which relates to the separate existence of vapour in the atmosphere, will prove equally unfortunate when brought to the test of experiment. Mr. Dalton, in all probability, supposed he had done all that the confirmation of this theory required, by inventing the doctrine of separate equilibria; for nothing more has been offered in support of his opinions, particularly of that relating to the existence of uncombined vapour pervading the atmosphere, unless the statement of the following experiment, with his explanation of it, may be referred to this head. If two parcels of dry air, which are equal in bulk, density, and temperature, be confined by equal columns of mercury, in two tubes of equal bores, one of which is wet and the other dry, the air, which is thus exposed to water, will expand more than that which is kept dry, provided the corresponding augmentations of their temperatures be equal; which phænomenon is thus explained on the principles of the theory: The vapour that arises from the sides of the wet tube, possesses a spring of its own; therefore it takes off part of the weight of the mercury from the air, and thereby leaves it to expand itself, so as to re-adjust the equilibrium. According to this explanation, if l and g represent the lengths of the columns of dry and moist air at any temperature; and if c denote the length of a column of mercury, equal in weight

to the pressure that confines the contents of the tubes; and if f be put for the spring of vapour of the same temperature measured by a column of mercury, we have $g = \frac{lc}{c-f}$; from which we also get $c = \frac{fg}{g-l}$: the last expression affords us an opportunity of comparing the preceding explanation, and therefore the theory itself, with facts; for, according to the experiments of Mr. Schmidt, 1000 parts of dry air, at 32 degrees of Fahrenheit, will expand to 1087·11 parts, by being raised to 59 degrees, in contact with water; call this number g; according to the same author, 1000 parts of dry air at 32 degrees will expand to 1053·61 parts, by being heated to 59 degrees in a dry tube: let this number be l; then $g - l = 33{\cdot}50$: but f, or the spring of vapour at 59 degrees, is 507, according to Mr. Dalton; then $fg = 551,164$; hence $c = 16{\cdot}15$ inches; which expresses the height of the barometer, together with the column of mercury contained in the tube. If the temperature be stated at 95 degrees, c will amount to little more than eight inches: now it is highly improbable that Mr. Schmidt made his experiments when the barometer stood at a height indicated by either of these numbers. This application of the theory to practice affords a presumptive evidence that the principles of it are not altogether just, supposing the experiments of Mr. Dalton and Mr. Schmidt to be correct; but a positive proof of a want of accuracy in these principles may be obtained by introducing a small change into the manner of conducting the experiment made with moist air. This alteration consists in discarding the stopple of mercury, and substituting the simple pressure of the atmosphere in the room of it; because, when this substance, which is impenetrable to steam, has been removed, the redundant vapour will, according to the theory, flow into the atmosphere, thereby leaving the moist air of the tube to follow the law of expansion observed by dry air. With a view to find whether this be the case or not, I filled a bottle with running water of the temperature of 59 degrees, which, when carefully poured out again, weighed 7794 grains. The bottle,

tle, having a dew left sticking to the sides of it, was placed in water at the temperature of 126 degrees: the mouth, which remained about an inch above the surface, was covered with my hand, care being taken to remove it frequently for an instant to permit the vapour and expanding air to escape. After keeping it in this situation about two minutes, I secured the mouth in the manner described above, and inverted it in a quantity of the same water, where it was reduced to 59 degrees; in consequence of which it took up 1622 grains of water, leaving a space equivalent to 6172 grains. If the experiment be now inverted, 6172 parts of air will occupy the space of 7794 such parts when its temperature is raised from 59 to 126 degrees; which is nearly double the expansion of dry air in like circumstances. For, according to Mr. Schmidt's experiments, 1000 parts of dry air of 59 degrees will become equal to 1133·03 such parts, by being heated to 126 degrees; therefore, by the rule of proportion, if 1000 parts give an expansion of 1133·03 such parts, 6172 parts give only 820: but the difference of 7794 and 6172 is 1622, which is nearly the double of 820. The preceding experiment, and others which I have made of the same kind, demonstrate that moist air expands more than dry air under like circumstances, and the fact subverts the notion of uncombined elastic vapour mixing with the atmosphere. The accuracy of the fact may be disputed; the doubt, however, is removed by repeating the experiment: but so long as my statement remains uncontradicted, the consequences of it to the theory in question cannot be controverted by argument: for if elastic vapour mix with the air, it does more than merely enter the pores of this fluid; for, according to my experiment, it enlarges these pores at low temperatures, which we know to be impossible, unless the heat of the compound arises to 212 degrees. Those who are convinced of the superior expansion of moist air, will readily apply the principle to certain interesting phænomena, in particular to the origin of tornadoes in hot countries, and the variation of the barometer in temperate climates.

Mr. Barrow, an intelligent traveller in South Africa, observes, that the atmosphere in Caffraria is sometimes heated

to

to 102 or 104 degrees: this is succeeded by local thunder storms, attended with heavy falls of rain and hail, as well as violent hurricanes. I do not pretend to assign the refrigerating cause, or the agent that produces precipitation in this case; I only have to observe, that the portion of air must lose much of its elasticity, which is suddenly cooled to 70 or 72 degrees, and at the same time parts with the water it held in solution. This partial diminution of spring will destroy the equilibrium of the adjacent parts of the atmosphere, and may be supposed to produce the tornadoes of the tropical regions. The same cause probably gives rise to the fluctuations of the barometer in milder climates; for, though the changes of temperature are less in the milder than in the hottest parts of the globe, the agents that precipitate the water of the atmosphere appear to act on a more extensive scale, and through a longer duration, in the former situations than they do in the latter. Wet weather is neither momentary nor local in Europe; provinces, and even kingdoms, are deluged with rain for weeks together. The air, which discharges such an abundance of water, will lose part of its spring, according to Mr. Schmidt's experiments, even when it suffers no change of temperature: now it is evident that the equilibrium cannot be restored in an instant, because the diminished elasticity must be augmented in this case by currents of air coming from remote places. The diminution of spring in the atmosphere is shown by the fall of the barometer, and the subsequent ascent of the mercury indicates the arrival of the restorative currents. According to this explanation the barometer will rise slowly but gradually in the centre of the rainy district, while the motions of it will be more rapid and less regular towards the verge of the storm. High winds will also prevail in wet seasons, which will blow towards the parts where the elastic force of the air is least; that is, where the rains are most abundant.—I know not what claim to originality is due to the foregoing hints towards the theory of the barometer; they have, however, the merit of being a natural consequence of an established fact; I mean the great dilatation of air saturated with moisture, which must undergo a proportionate contraction when deprived of water.

XX. *An experimental Inquiry into the Nature of Gravelly and Calculous Concretions in the Human Subject; and the Effects of Alkaline and Acid Substances on them, in and out of the Body.* By Thomas Egan, *M.D. M.R.I.A.*

[Concluded from p. 86.]

Experiment XI.

As children are such frequent sufferers, Mr. Richards suggested the propriety of ascertaining whether the alkaline influence might be weakened by the addition of sugar. One-half of a calculus, of the uric acid kind, weighing 185½ grains, extracted by my friend Mr. Richard Dease, and (though under the most unpromising circumstances) with a dexterity and success not to be exceeded by his late father, was suspended in a lixivium consisting of eight ounces of distilled water and twenty drops of weak *aqua kali puri* (partly aërated), and scarce imparting an alkaline taste. To this were added thirty-six grains of sugar, which were found adequate to sweeten it sufficiently. After remaining forty-eight hours in a temperature varying from 55 to near 100 degrees, or a medium one of 74 degrees, being dried and weighed, it was found to lose ten grains three quarters. The addition, then, of saccharine matter cannot diminish, but may add to the alkaline energy.

Experiment XII.

Ten grains of very pure crystallized carbonate of potash were dissolved in four ounces of distilled water. In this filtered lixivium was suspended a fragment of calculus, of the uric acid kind, weighing seventy-two grains and a quarter, for forty-eight hours, on a sand-heat, varying from 50 to 100 degrees (for the fire was not kept up during the night). Being taken out, dried, and weighed, it was found to have lost seven grains and a quarter. The solution had a yellowish green colour, different from the light yellow tinge of the pure alkaline ones. It also lost its taste, but without becoming sweet. A quantity of flocculent animal matter was separated, and the dissolved uric acid was, for the

the greater part, again precipitated, upon the mixture cooling, to the temperature of the atmosphere.

Experiment XIII.

The crystallized carbonate of potash, being generally prescribed in the proportion of one drachm to four ounces of water; in a similar mixture was suspended an entire calculus, of a very compact, rough, and gritty appearance, weighing forty grains and a quarter. After remaining forty-eight hours in the above temperature, it was taken out, dried, and weighed, and found to have lost three grains three quarters. The solution here more highly coloured than in the former: some spontaneous precipitation; and an immediate one, on the addition of a few drops of weak marine acid. We then find the vegetable alkali in the fullest state of saturation, with carbonic acid, that we can procure it, in the solid form, acting powerfully on these concretions, when assisted by degrees of temperature even much inferior to that of the human body.

Now, as to the mineral alkali, nature presents us with similar, nay, more extraordinary results, in the mild mineral alkaline impregnation of the waters of Carlsbad, in Bohemia. Here are several springs, varying in temperature from 114 degrees to that of the Brudel at 165 degrees. According to Elliot, they contain, in the gallon, of aërated lime 36 grains; muriate of soda 48; aërated soda 102; vitriolated soda 6 drachms; some minute proportion of iron, and a considerable carbonic acid impregnation. But Klaproth rates the proportion of mineral alkali still higher.

Of the lithontriptic effects of these waters, Springfield gives us a very surprising account indeed: founded, however, upon numerous experiments, instituted upon the spot, by the immersion of many calculi in the sources themselves; where they were either entirely dissolved, or acted upon with an energy that must appear incredible, if we did not consider the nature of the menstruum, its high temperature, and constant renewal by the flowing of the stream. Nay, the urine of patients who used these waters for a few days was found to possess powerful lithontriptic effects, as appeared by the

immersion of many calculi in it. For an account of these highly interesting experiments, too numerous for insertion here, I must beg leave to refer to his Treatise *De Prerogativa Thermarum Carolinarum, in dissolvendo Calculo Vesicæ, præ Aqua Calcis vivæ.*

From these experiments, as well as the highly beneficial effects of these waters, taken internally, by the numerous calculous and gravelly patients who frequent Carlsbad, he establishes their superiority over the different alkaline and other remedies hitherto in use, not excepting Whyte's oyster-shell lime water. Now, the lime in these being carbonated, and only kept in solution by their highly aërated state, we can be at no loss, in those days, to attribute their superior agency to the alkaline impregnation, assisted by so high a temperature. Klaproth affirms, that a person who drinks these waters, in the usual quantity, for twenty-six days, takes of mild mineral alkali 3913 grains, or 8 ounces 1 drachm and 13 grains; which amounts to two drachms and a half per day, besides the other saline ingredients.

Doctors Rutty and Smyth, who gave us a valuable extract from this publication, in the Memoirs of the Medical and Philosophical Society of this city, (now in the library of the Royal Irish Academy, but which, we have sincerely to regret, were never published, and are now discontinued,) conclude their account by the following query: "May not some alkaline lixivium be contrived by art, that would possess similar effects with these waters?" And has not this partly taken place in the instance of our soda waters? But may we not make a nearer approximation by a solution of the above specified proportion of mineral alkali in the relative quantity of water, with the addition or omission of the carbonic acid, and the other saline ingredients, as may be thought proper, afterwards heating, however, each separate dose to 160 degrees?

We find, then, the alkaline carbonates, in the great laboratory of nature, as well as in our experiments, exerting considerable solvent powers upon these animal concretions contrary to what has been hitherto supposed.

Experiment

Experiment XIV.

Into a filtered solution of ten grains of salt of tartar, in four ounces of distilled water, were introduced two fragments of calculi, weighing seventy-four grains and a quarter. The mixture was set aside for forty-eight hours in a cool room; temperature varying from 47 degrees at night, to 55 degrees in the day. After twelve hours it began to be coloured, and continued to be more so, until the temperature fell to 51 degrees, when a precipitation took place, and continued during the night; so that it appeared to deposit at the temperature of 47 degrees, what was taken up at degrees somewhat exceeding 51°. These fragments, on being taken out, dried, and weighed, were found to have lost three grains and three quarters; the laminæ disposed to crack, and the strata to separate and crumble. This weak lixivium, then, exerted much energy, even in a very low temperature.

Experiment XV.

A fragment of calculus, weighing seventeen grains three quarters, was immersed in a lixivium of similar strength; but now exposed to a temperature varying from 51 degrees at night to about 95 degrees in the day. After forty-eight hours, it was found to lose five grains and a half: a prodigious quantity, when we consider the small surface presented by this fragment, weighing only seventeen grains three quarters. The solution, upon cooling, became turbid as before, and precipitated a large proportion of the dissolved uric acid.

Experiment XVI.

A fragment of calculus, weighing forty grains three quarters, was immersed in four ounces of soda water for forty-eight hours, and exposed to a temperature varying from 55 to about 100 degrees. Its loss amounted to one grain. A repetition of this experiment afforded nearly the same result; and demonstrates, that though the soda, in this supercarbonated state, still exerts some energy on concretions of the uric acid kind, yet it is but feeble; and that these waters appear more capable of preventing their formation than ef-

fecting their solution, when they once acquire the aggregate state. The same fragment, in a similar quantity of soda water, in the temperature of from 50 to 55 degrees only, sustained no loss, after forty-eight hours. And here we have another proof of the necessity of seriously attending to the degree of temperature in all researches of this kind.

But it may be observed, as to the internal use of alkaline substances in particular, that their effects must be considerably weakened upon their immediate admixture with the urine; as the small quantity that can be conveyed there must, in the first place, neutralize the uncombined phosphoric acid in all urine, the benzoic in children's, and decompose the ammoniacal and magnesian phosphates in that of every period of life. It must be acknowledged its efficacy is partly counteracted by these circumstances, which should never be overlooked, and always taken into account in practical application. Referring to Fourcroy's instructive essay on this subject, Memoirs of the National Institute, and *Connoissances Chimiques*, let us here once more appeal to the test of experiment.

Experiment XVII.

A fragment of calculus, weighing eighteen grains one quarter, of the uric acid kind, was suspended, for forty-eight hours, in an alkaline lixivium, consisting of four ounces and a half of recent urine, and twenty drops of a very weak, and partly aërated, caustic lixivium; medium temperature about 74 degrees. On being taken out, and dried, it was found to have lost one grain three quarters; a considerable quantity from so small a specimen. To the filtered solution were added a few drops of dilute marine acid, which, after a few minutes, precipitated a reddish crystalline matter in a triple proportion of what generally occurs in the natural state of urine.

From the above experiments, therefore, it appears no longer doubtful; first, that pure lime, even in the small proportion contained in lime water, and the pure alkalies, in an extreme state of dilution, in temperatures even somewhat inferior to those of the human system, exert an active solvent power on calculi of the uric acid kind: secondly, that

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the alkaline carbonates, under similar circumstances, are possessed of similar powers, though in an inferior degree: and thirdly, that, by our having ascertained this point, we have removed a long established error, substituted a discovery highly interesting to animal chemistry, and likely to be productive of a more enlightened and successful practice in the treatment of these diseases.

In these expectations we shall appear to be the better founded, when it is considered, that, for want of entire specimens (preserved here like the oriental bezoars of old), we were obliged to operate upon fragments presenting small surfaces only to our solvents: that these last were never renewed during the course of the experiments, which would not have occurred in their application in the form of injections; as they should, in that case, be so often repeated, and act, of course, with renewed energy: that, either taken internally, or used in form of injection, the smallest proportion of alkaline matter, in a great state of dilution, assisted by the human temperature, answers our purpose; and that the temperature in our experiments was never permanent, and might be rated at the medium one of 74 degrees.

Having now fulfilled the second object of this essay, I would no longer presume to trespass on the indulgence of the academy, if I were not actuated by the sanguine hope of turning the attention of my surgical friends to the humane consideration of obviating, as much as possible, the most dangerous of operations by the prudent application of a few safe solvents injected into the bladder. How far they may succeed with calculi of the uric acid kind, may be already conjectured from the preceding experiments; but with those of the next most frequent occurrence there is much less difficulty to encounter, and every reason to hope for a speedy and safe result. The ammoniaco-magnesian phosphate is partly soluble in water, highly so in the carbonic acid (as we have already seen); and, consequently, more so in the weakest possible acid impregnations that can be devised; nothing more being necessary than the addition of as many drops only of weak muriatic acid as will scarce impart an acid taste. But as precept should, in every instance, be as

much as possible assisted by experiment, I shall, for the encouragement of the young practitioner, exhibit a few on this very soluble species the more willingly, as he has no assistance to expect from his professional books; these subjects being only treated of in Philosophical Transactions, Memoirs of the National Institute, and a few other foreign chemical publications, if we except Whyte's Treatise on Lime Water, to which we would willingly refer him.

Experiment XVIII.

An entire calculus, of a reddish, gritty appearance, externally, proved to consist of ammoniaco-magnesian phosphate, weighing forty-six grains one quarter, was suspended, for forty-eight hours, in a mixture consisting of four ounces of distilled water and ten drops of weak marine acid. After being taken out, and dried, it was found to have lost six grains three quarters. The mixture was whitish, lost its acid taste, and precipitated, on the addition of a few drops of fixed alkali, the ammoniaco-magnesian phosphate, under that beautiful crystalline form so accurately described by Dr. Wollaston.

We may readily conceive how much more the loss would have amounted to in this case, in the short space of forty-eight hours, if the menstruum had been frequently repeated under the regular influence of human temperature.

Experiment XIX.

A fragment of the same species with the above, weighing twelve grains, was immersed, for forty-eight hours, in three ounces of distilled water, without addition: temperature from 60 to near 100 degrees. After being taken out, and dried, it was found to have lost one grain three quarters, became so friable as to crumble, and the solution to precipitate with a few drops of pure ammonia. This species of calculus, therefore, is soluble in water, at temperatures even inferior to that of the human. It is unnecessary I should enter into a further detail of experiments made upon calculi of the mixed kind, having the uric acid, phosphate of ammonia, and sometimes, though rarely, phosphate of lime, intermixed

intermixed in their strata. Suffice it to say, that the very dilute marine acid speedily takes up the earthy phosphates, leaves the laminæ of the uric acid bare and distinct, ready to crumble, and of easy solution in the weakest alkaline lixivia, and still more so in lime water:—a most important consideration in a practical point of view.

It would be trespassing too much on the already tried indulgence of the academy, to go further into the detail of the circumstances necessary to be attended to, and acquainted with, to ensure success in the application of these principles. These are already tolerably well detailed in the *Connoissances Chimiques*. To the gentlemen professors in the school of surgery it more particularly belongs; and from the zeal and talents now in full activity there, what may not be expected? Created only the other day, by a Cleghorn, (a name as deservedly as universally revered;) fostered, afterwards, by the anxious care and talents of Mr. Dease; we find it already arrived at a state of perfect maturity, and holding out to the student advantages no where to be rivalled, if indeed equalled: and that nothing may be wanting to a complete medical as well as surgical education, establishing a chair of botany, supported by the acknowledged abilities of Dr. Wade, both as a botanist and teacher. From the above experiments and observations we may presume to draw the following conclusions:

That acids, and acescent drinks of all kinds, give rise to gravelly and calculous affections, by causing a separation and precipitation of the native uric acid of urine within the body. That all acids, vegetable or mineral, nay, the native phosphoric acid of urine, in excess, are equally productive of this effect; the tartaric, perhaps, somewhat more so. That, on the other hand, we find lime, both the fixed alkalies, pure as well as aërated, (even in the smallest proportions,) serviceable in these disorders, by uniting with, and keeping in solution, this acid substance. That they also, in the smallest proportions, and diluted state, exert strong solvent powers on this acid in its aggregate form of calculus, provided their action be favoured by degrees of temperature approaching to the human. That, under the

same

same circumstance, contrary to what was generally supposed, the carbonated, sub-carbonated, nay, the super-carbonated, exert similar influence, though in an inferior degree. That lime, even in the small proportion it presents itself to us in lime water, is a most active and safe solvent of calculi of the uric acid kind, and its various combinations; as has been long since ascertained by Whyte. That, weight for weight, it exceeds even the caustic alkali in any state of dilution that the latter can be applied to the living body. That, finding four ounces of lime water, containing only two grains three quarters, take up, or detach, seven grains three quarters from a very compact calculus, we may be led to suppose this may arise from its action on the agglutinating medium, its affinity to, and energy on, animal matter being so well known; and, if so, may we not expect something from its power on the mulberry calculus, our most formidable enemy? For, though it cannot touch the oxalate of lime, it may the cementing medium, with which it peculiarly abounds.

For the application of these established facts to useful purposes, I must refer to my surgical friends, being all now possessed of the necessary degree of chemical acquirement; and I am happy to find this career already entered on by my friend Mr. Crampton, who has favoured us with an analysis of a pulmonary calculus in the Philosophical Transactions, and from whose professional as well as scientific talents we have every thing to expect in fulfilling (even on this occasion) his duties as a teacher.

Having now endeavoured to accomplish the chief object of this essay, which was, to establish experimentally a more clear and comprehensive view of the nature of these maladies, and the remedies employed to combat them, than we hitherto possessed, I should not have trespassed further on the time of the academy, were it not properly suggested, by my friend Dr. Clarke, that it would be of importance to ascertain how far the facts and notions, brought forward in it, may stand confirmed or contradicted by the result of our practical application of them in Simpson's Hospital; an establishment affording the best and most extensive field of observation,

observation, of this kind, of any in Europe, that of Luneville, perhaps, excepted.

The benefit of this charity extends equally to the blind and gouty. In the year 1795 I found it to contain thirty-two of the latter; and since that period thirty-four have been admitted: in all, sixty-six gouty patients. Of these, the greater number have either complained of gravel, or passed it without any previous or concomitant inconvenience: a circumstance which I had every day occasion to observe, whilst attending to the state of gouty urine. Among the blind and gouty, however, we may count about twenty-two as specifically more afflicted, having occasionally complained of marked and distinct symptoms of this disorder. Of these, we find sixteen among the gouty, and six only among the blind. Now, as the severity of gout is uniformly diminished, nay, in many instances, the disease entirely removed, by a residence of a few years only in the house, we must expect to find the same take place with respect to gravel, to which it is so strongly and nearly connected. And this singular alleviation of both diseases we can only attribute to the influence of temperance, and the manner of living, very opposite to that of their former habits. The diet in our house consists of bread and milk for breakfast and supper; beef, or mutton, with table beer, for dinner; all of the best quality, and administered with the greatest propriety and regularity; whilst the introduction of ardent spirits is prohibited, and sobriety enforced, by the strict discipline of the house. On the other hand, we find that, previous to their admission, they were either addicted to intemperance, or in the habit, at least, of muddling in public-houses, where, after a libation with porter, they indulged in the free use of acidulated punch (the constant nocturnal practice of our middling tradesmen and shopkeepers, who furnish the greatest proportion of our patients). The keeper of a porter-house of considerable resort informs me, that, to please the generality of his customers, he finds it necessary to add the juice of an entire lemon to about two quarts of punch; and that, from this circumstance, he would have experienced a considerable di-

minution of his profits, if he did not occasionally substitute cream of tartar, or the dilute sulphuric acid: an innocent and safe practice, in his opinion. Now, so satisfied are our patients of the pernicious effects of acids of all kinds, that we find many of them refuse to make use of our table beer during the summer months, through the apprehension of its acescent quality (as was before observed), and which continued to be the practice of Hewson, Khensk, Clapham, and others, for years back: nor do our present two greatest sufferers, Sing and Cox, venture on it at any season but with the greatest caution.

To a removal, then, from the former occasional causes we may attribute no small share of the alleviation of those diseases which takes place with us: a practical observation, that cannot be too generally known. But to return to my subject:—On the slightest appearance of gravelly symptoms, unconnected with fever, or inflammatory tendency of the urinary system, our patients have recourse to an alkaline medicine, the gravelly pills (as they term them), which consist of desiccated soda, in the most convenient form for hospital practice, as well as most suited to gouty stomachs. Of this (as first advised by Beddoes,) one drachm, with the addition of a few grains of capsicum, or drops of essential oil, and the necessary quantity of hard soap, or extract, is made into twenty pills. Of these, from three to six, or more, are taken in the twenty-four hours; and are found sufficient, not only to alleviate or remove these complaints, but even to render the interference of the physician but seldom necessary. We have had also occasion to remark, that several of our patients, induced by their marked beneficial effects, carried these pills about them, so as to have occasional recourse to them, without much attention to either dose or number.

To this practice, then, we would be disposed to attribute the very pleasing and interesting consideration, that, among so many gravelly patients, there has not occurred, in the course of ten years, a single operation of lithotomy; nor has the catheter, even in the hands of our expert and able surgeon, Mr. Macklin, been able to discover the smallest occasion

occasion for it. We could therefore have no opportunity of ascertaining the efficacy of injections into the bladder, as recommended by Whyte, Fourcroy, and myself.

I shall conclude by observing, that it would be interesting to have it in our power to extend these researches to the urine of those who live habitually on different aliment and drinks, particularly of the acescent kind, as well as to that of those who drink waters with mineral alkaline impregnations. But this desirable object can be only obtained by the concurrent exertions and attention of gentlemen of the faculty in different countries and situations. In private practice it is not to be expected; for here, wherever experiment is surmised to be the object, mistrust and suspicion take place of professional confidence. The use of the nitric acid in our venereal hospital, I hoped, would afford some useful facts as to its effects upon the saline contents of urine; the uric acid in particular. But I had not, as yet, sufficient leisure for that inquiry; nor could I, hitherto, obtain the urine of those using it, with all the circumstances necessary to enable me, at this moment, to draw any direct conclusions from my examination of it. In many instances, a morbid state of the urinary system (the urethra in particular) took place. In others, the combined effects of mercury interfered: and in all, no certainty of its not being blended with the urine of others not using this acid. I could not, however, help observing, that the few specimens, sent to me, agreed in one particular, viz. their exceeding very little, if at all, the usual healthy standard of acidity. This circumstance must excite our attention the more forcibly, when we consider, that two drachms of nitric acid, nay, sometimes three, diluted in the proportion of one pint of water to each drachm of acid, were taken daily; whilst, on the other hand, a few drops of the acid elixir of vitriol, or tincturæ martis in sp. salis, nay, the weak vegetable acids, and cream of tartar, persevered in for a few days, impart an additional degree of acidity to the urine. Would not this observation (if founded), conjoined with the easy decomposable nature of the acid itself, and its action on animal matter, induce us to lean to the opinion of those who have already asserted that this acid

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is partly decomposed in the system, imparts its oxygen to it, and that, perhaps, to a degree capable of annulling or destroying its properties as an acid?

And it may be here further remarked, in confirmation of such notion, that those gentlemen most conversant with it here, as well as most capable of judging, entertain strong doubts of its supposed diuretic effects, allowance being made for the necessary quantity of its watery vehicle. If it be, then, truly deoxygenated in the system, why be deterred by its failure, as a radical cure of siphylis, from extending our trials with it here, to other chronic diseases, as they have already done in India?

XXI. *Extract of a Memoir of Messrs.* FOURCROY *and* VAUQUELIN *upon Guano, the natural Manure of the South Sea Islands near the Coast of Peru. Read before the National Institute. Drawn up by* A. LAUGIER*.

AMONG the vast number of objects worthy of the attention of naturalists which M. Humboldt has observed and collected during his late travels, *guano* is one of the greatest importance. In making us acquainted with this singular substance, one of the principal resources of agriculture in the countries he has visited, this celebrated naturalist has furnished the authors of this memoir with an opportunity of confirming a discovery which they had made at the very moment of M. Humboldt's return. The perusal of their memoir upon the existence of uric acid in the excrement of birds, created the idea in Humboldt that the guano, found in the islands on the coast of Peru frequented by a great number of birds, might be of the same description. It belongs to chemistry alone to decide the degree of credit to which this conjecture is entitled. Messrs. Fourcroy and Vauquelin have undertaken the examination of this substance, and we purpose laying before our readers the results of their labours as inserted in the Memoirs of the National Institute.

* From *Annales de Chimie*, vol. lvi. p. 258.

Before

Before giving an account of the experiments made upon guano in order to discover its nature, it may not be improper to communicate M. Humboldt's own opinion, as conveyed in a note to the authors of this memoir.

" Guano is found in great abundance in the South Sea, on the islands of Chincha, near Pisco; but it exists also on more southern shores and islands, such as Ilo, Iza, and Arica. The inhabitants of Chancay, who make a traffic of guano, go and return from the islands of Chincha in twenty days. Each vessel has on board from 1500 to 2000 cubic feet. A vanega costs at Chancay 14 livres, at Arica 15 livres tournois.

" It lies in beds of 50 or 60 feet deep, which are wrought like mines of iron ochre. These islands are inhabited by an immense quantity of birds, particularly of the ardea (the heron) and the phœnicopterus (flamingo) kinds, which always pass the night on shore; but it requires a period of three centuries before their excrements increase to four or five lines in thickness. Is guano a production caused by changes which the globe has undergone, like coal or fossil wood? The fertility of the otherwise steril shores of Peru is produced by the use of this substance, which is a staple article of commerce. Fifty small vessels called *guaneros* are continually employed in searching for it, and transporting it to the main land; and we may perceive the smell of it at a quarter of a league's distance. The sailors, who are accustomed to this ammoniacal odour, suffer no inconvenience from it, but it caused us to sneeze violently on approaching it. For the cultivation of mace, in particular, it furnishes a most excellent manure. The Indians have taught this method to the Spaniards. If too much guano, however, is thrown upon the mace, the root is burnt up, and destroyed. Guano is extremely acidifiable; so that here we have a manure of hydruret of azote, while other manures are rather hydrurets of carbon."

Guano has a reddish yellow colour; it is almost tasteless, but has a strong smell, which resembles at once that of castor and valerian. It blackens in the fire, exhaling a white smoke and an ammoniacal odour.

Its

Its solubility in water, and, above all, in potash, determined the method to be taken in its analysis. They successively tried it by means of water, potash, and muriatic acid: each of these trials gave occasion to observe several phænomena, of which we shall give a succinct account without entering into the details, which may be too extensive for an extract.

Ten grammes of this substance, repeatedly washed in great quantities of boiling water, were reduced to 5·7 grammes. The ley had a reddish colour, and reddened turnsole paper.

Submitted to distillation in B. M., it furnished ammonia during the whole operation: 24 hours afterwards it deposited a reddish yellow powder, a little sapid, having the odour of castor; and it presented on its surface a crystalline pellicle of the same colour as the precipitate.

The liquor, filtered and evaporated a second time until it was reduced to 3 grammes, still deposited, on cooling, a reddish yellow powder, like the first, but less abundant.

The yellow powder and the mother water which held it in solution were examined separately.

The former presented the following properties:—It is a concrete and pulverulent substance, of a brilliant and crystalline appearance, and of a reddish yellow colour. Exposed to heat, it burns entirely, and gives out a slight empyreumatic odour of ammonia and prussic acid. Although little soluble in cold water, it is easily so in warm, to which it communicates a yellowish colour; and, although the solution is tasteless, it reddens the tincture of turnsole; it precipitates the solutions of acetate of lead and of nitrates of silver and mercury in coloured flakes, which the nitric acid redissolves completely.

This substance dissolves instantly in an alkaline ley, to which it communicates a brown colour, exhaling a lively odour of ammonia. Sulphuric acid, mixed with a concentrated alkaline solution, yields a very thick whitish precipitate, and liberates a sharp smell similar to that of weak acetic acid.

The celebrated authors of the memoir concluded from these experiments that this powder presents itself as an acidulated

dulated salt, formed from an animal acid, ammonia, and a little lime. In short, very weak nitric acid, in which this salt had been macerated to detach its acid from its bases, yielded, upon evaporation, vapours of ammonia in abundance by the addition of potash, and unequivocal signs of lime by the addition of oxalic acid.

Freed from ammonia and lime, this substance is less coloured, and appears less soluble than before: its solution in boiling water deposits brilliant crystals tolerably hard, and reddens more strongly turnsole paper. It combines with potash easily, and without the smell of ammonia: all the acids separate it from potash. It blackens by heat, and burns without leaving any residue, giving out a smell of ammonia and prussic acid. Its neutral combination with ammonia does not precipitate the solution of sulphate of alumine, as the lithistic acid does.

It results from all these facts, 1st, That the substance extricated from guano by boiling water is an acid partly saturated with ammonia and a little lime: 2d, That this acid is of an animal nature, since it yields ammonia and prussic acid on being decomposed by fire: 3d, That this acid, according to all the properties already indicated, is uric acid, and similar to that of the excrements of aquatic birds: 4th, That it forms about the fourth part of guano.

The mother water which deposited the powder, the properties of which are about to be detailed, is very acid: potash liberates from it ammonia in abundance; it therefore contains an ammoniacal salt: the nitrates of barytes and silver announce the presence of the muriatic and sulphuric salts in it also: lime water precipitates from it white flakes, difficult of solution in the muriatic acid.

This precipitate, occasioned by lime water, is evidently formed of two salts: both dissolve in the acids without effervescence: the one is easily dissolved, without the assistance of heat; the other is dissolved with difficulty, even by the assistance of heat. The former resists calcination; the latter is decomposed by fire, and afterwards dissolves in the acids with effervescence. The one is phosphate and the other oxalate of lime.

The weak nitric acid was made use of by Fourcroy and Vauquelin with an intention of separating these two salts without making them experience any alteration. It dissolved the phosphate of lime, without touching the oxalate. This last salt, tried with a solution of carbonate of potash, yielded a precipitate which dissolved with effervescence in the nitric acid: this solution evinced all the properties of nitrate of lime. The acid separated from the lime united with the potash. In short, the liquor presents the characters of the oxalate of potash. Lime water precipitated from it a very subtle powder. Sulphate of lime precipitated very light flakes, and precipitates were formed by all the metallic solutions which precipitate oxalic acid. Sulphate of alumine did not form any precipitate, as might have happened with *honistate* of potash.

The potash which the illustrious authors of the memoir found in the mother water of guano after its precipitation by means of lime water, the liberation of ammonia which they obtained by the addition of potash in the mother water before its decomposition by lime water, sufficiently prove that these two alkalies saturate the acids contained in the mother water of guano: thus this mother water certainly contains oxalates, phosphates, sulphates, and muriates of potash and ammonia.

The five grammes and seven-tenths remaining after the action of the water upon the ten grammes of guano submitted to the analysis, were tried by caustic potash, which took eight-tenths from them: this alkaline solution contained nothing else than the uric acid, and a small quantity of fatty matter.

The muriatic acid, to the action of which the four grammes and nine-tenths not attacked by potash had been submitted, did not present upon analysis any thing else than phosphate of lime, iron, and an atom of carbonate of lime.

The water, the potash, and the muriatic acid, successively employed, left no other residue out of ten grammes of guano than three grammes, and a tenth part of a matter which was recognised to be a mixture of a quartzy and ferruginous sand.

The

The result of this interesting analysis is; that the South Sea manure is formed,

1. Of uric acid, which forms the fourth part, and which is partly saturated with ammonia and lime.

2. Of oxalic acid, partly saturated with ammonia and potash.

3. Of phosphoric acid, combined with the same bases and with lime.

4. Of small quantities of sulphates and muriates of potash and ammonia.

5. Of a little fatty matter.

6. Of sand, partly quartz and partly iron.

The existence of guano in the places where such multitudes of birds assemble, the identity of its nature with that of the excrements of aquatic birds, necessarily throw great light upon the origin of this substance.

Its analysis proves how well founded the ingenious naturalist is in his opinion, to whom we are indebted for our knowledge of this substance, equally interesting to us as useful to the inhabitants of Peru. The analysis, also, confirms the important discovery, which is the happy fruit of the researches of Fourcroy and Vauquelin. In short, it has the advantage of recalling this well known truth, that the sciences mutually enrich each other, in aiding each other with the various lights they possess; and it furnishes a new occasion to remark, that among all the sciences none have a more immediate and more necessary connection than chemistry and natural history.

XXII. *Analysis of Birdlime. By M.* Bouillon-Lagrange*.

I. *Origin and Mode of Preparation.*

The substance termed birdlime is classed among the immediate products of vegetables. M. Fourcroy is the first who regarded it as glutinous: he has placed it as a species

* From the *Annales de Chimie*, tom. lvi.

under this general head in his *Systême de Connoissances Chimiques*, vol. vii. p. 306.

Birdlime, says this chemist, is procured from the fruit of the misletoe and the tender bark of the holly, as well as from many other trees, by maceration in water. Although no one has hitherto examined this substance with sufficient accuracy, it presents many points of resemblance to glutinous bodies.

With the exception of what is said in my Manual of a Course of Chemistry, I know no work in which any light is thrown upon the nature of this singular matter.

M. Chaptal, in his Elements of Chemistry, speaks only of its preparation: as the process which is there detailed differs in nothing from that which is found in the Materia Medica of Geoffroy, and in the Dictionary of Valmont de Bomare, I shall quote the article respecting the preparation of this substance:—"The antients employed the berries of the misletoe of the oak: they boiled the fruit in water, then bruised them, and strained off the warm liquor, so as to separate the seeds and skins. But we now prefer the birdlime procured from the bark of the holly: the inner bark, which is the tenderest and greenest, is made choice of, and allowed to putrefy under ground; it is then beaten in mortars to reduce it into a paste, which is washed and kneaded in water. This substance was considered as resolvent and emollient when applied externally."

We know also that the misletoe of the oak forms an ingredient in many pharmaceutical preparations, such as the universal water, the antispasmodic powder, and Guttet's powder.

The English, according to Geoffroy, prepare their birdlime from the bark of the holly. They boil this bark, says he, in water for seven or eight hours, until it has become tender. It is then formed into balls, which are placed under ground, in numerous layers, one above another, with pebbles interposed; the water being previously all drained off: there they are suffered to remain to ferment and putrefy for a fortnight or three weeks, until they have been converted into mucilage. They are next removed and pounded in a mortar till

till they can be worked like paste; after this they are washed in a stream of water, and kneaded together, so as to remove all foreign matters. The paste is now deposited in earthen vessels for four or five days, till it throws up a scum and purifies. It is then put up in proper vessels, and is ready for sale.

This mode of preparing it is not generally adopted; each county has its own process, and there are even some persons who keep their method secret.

At Nogent-le-Rotron birdlime is prepared from the shreds of the second bark of the holly: it is allowed to ferment in a moist place for fifteen days, and then boiled in water, which is afterwards evaporated.

At Commercy and in its vicinity this substance is procured from different shrubs, as the holly, the *viburnum lantana*, and the misletoe; from every different tree, such as the apple, the pear, the lime, &c.

The best is obtained from the prickly holly, and is of a greenish colour: that which is made from the viburnum lantana is yellowish. When this vegetable is employed, they uniformly reject the epidermis, and use only the second bark.

I prepared the birdlime which I used in my experiments from the inner bark of the holly. On comparing it with some which was made with great care, and sent me from Commercy, I was unable to discover any sensible difference. These precautions appeared to me necessary, that a greater degree of accuracy might be given to the analysis. We well know that the birdlime of commerce is seldom pure; it is often a mere mixture of vegetable and animal matter, and often adulterated with turpentine, oil, vinegar, &c.: it is therefore absolutely necessary to ascertain previously the purity of this substance; and the process which I adopted yielded me birdlime of the best quality.

I took the inner bark of the holly, bruised it well, then boiled it in water about four or five hours, and, having thrown out the water, placed the bark in earthen pots under ground: I allowed it to remain there till it putrefied, or rather became viscid, taking care to sprinkle it with water

from time to time. When this process was completed I washed it, so as to remove all foreign matters.

II. *Chemical and Physical Properties of Birdlime.*

Birdlime has a greenish colour; its taste is bitter: it is glutinous, ropy, and tenacious: its odour resembles that of linseed oil.

When spread upon a plate of glass, and exposed for some time to the air and light, it dries and becomes brown. In this state it is no longer viscid: when it is completely dried it may be reduced to powder; it has then lost all its glutinous properties, and it cannot resume them by the addition of water.

Birdlime reddens the tincture of turnsole. When heated alone in a porcelain cup it melts without becoming very liquid; it swells up and forms bubbles, which rise to the surface and then burst. This kind of fusion discovers small black grains, which give it an uneven appearance: it diffuses an odour very similar to that of the fat oils when their temperature is raised.

If the birdlime be kept in fusion for some time, it acquires a brownish colour; but again resumes all its properties on cooling. When placed upon red embers it burns with a flame, and emits an abundance of smoke.

Heated in a crucible of platina, it burns as soon as the crucible becomes red. Its flame is bright, and rises above seven inches: it is attended with a considerable quantity of smoke, which is readily condensed upon the vault of the chimney. The combustion continues even after the crucible is removed from the fire. There remains a white cinder, which is very alkaline, and partly soluble in water. By the use of re-agents we ascertain the presence of sulphate and muriate of potash.

The portion which is insoluble in water, when treated with the muriatic acid, dissolves with effervescence.

The solution is copiously precipitated by the oxalate of ammonia; prussiate of potash produces a blue precipitate; and with ammonia it lets fall a matter of a pasty consistence, which is partly soluble in caustic potash: from these facts

facts we should be led to conclude, that the ashes contain, besides the salts soluble in water, a quantity of the carbonates of lime and alumine, with a little iron.

Water has very little action upon birdlime. When raised to boiling, the matter does not completely melt; it acquires rather more fluidity, but resumes its former consistence by cooling. The water has no colour; its taste is nauseous, and then bitter: it reddens the tincture of turnsole. When evaporated to the consistence of syrup it becomes coloured, and assumes a mucilaginous appearance. The admixture of alcohol separates this matter.

Water, therefore, only dissolves a mucilaginous substance, and a little extractive matter.

Caustic potash, on the other hand, has a very different action. Its concentrated solution immediately forms with birdlime a whitish magma, which becomes brown by evaporation: there is at the same time a disengagement of ammonia.

The compound formed is less viscid: it acquires a greater hardness by exposure to the air: its odour and taste are similar to those of soap.

It is completely soluble in alcohol, with the exception of a few vegetable remains. These solutions become muddy on the addition of the strong acids, and exhibit the other phænomena which are observed in the solutions of soap.

The weaker acids soften birdlime, and partly dissolve it: but when concentrated they act in a different manner. Sulphuric acid blackens and chars it: if powdered lime be added so as to form a thick magma, a disengagement of acetic acid and ammonia takes place: there is no doubt that, besides the free acetic acid contained in the birdlime, a new quantity is formed by the action of the sulphuric acid.

The nitric acid has little action in the cold upon the substance under examination; but if the temperature be raised it becomes yellow, melts, and as the evaporation proceeds the matter swells considerably, and a hard brittle mass remains. When submitted a second time to the action of the nitric acid, a solution is effected, and a part of this substance

is converted into the malic and oxalic acids. By continuing the evaporation we obtain a yellow and very friable matter, which softens between the fingers like wax, possessing at the same time a degree of elasticity, and melts with a gentle heat.

Potash combines with this matter, changes its yellow colour to a brown, and forms with it a true soap.

Alcohol dissolves it in part; forming a yellow solution, which loses its transparency on the addition of water.

If the alcohol be evaporated to dryness, a yellow substance remains, which has no longer a greasy appearance, and diffuses an agreeable odour when burnt.

Muriatic acid has no action on birdlime in the cold, but, when heated, blackens it.

Oxymuriatic acid, on the other hand, has a very different and more powerful action on this substance. On passing the gas through water containing liquefied birdlime, or by shaking it in a flask along with very strong acid, we observe the following phænomena:—the birdlime speedily loses its colour, becoming white; it is now no longer viscid, and separates into hard compact masses, which contain within them a portion of the substance unaltered. This incomplete oxygenation may be ascribed, with great probability, to the difficulty of preserving the birdlime liquid in hot water, and the consequent obstacle to the action of the acid beyond the external layer.

The characters of oxygenated birdlime are: 1st, It may be readily reduced to powder: 2d, It is insoluble in water even when heated: 3d, It refuses to melt at an elevated temperature: 4th, It does not become yellow, or form resin, when subjected to the action of the nitric acid.

The acetous acid softens birdlime, and dissolves a certain portion of it: the solution has a yellow colour and a nauseous taste: with the carbonate of potash it lets fall no precipitate: when evaporated it yields a matter which has the properties of a resin, but cannot be reduced to a state of complete dryness.

Some of the metallic oxides are readily reduced when heated with birdlime.

The

The semi-vitreous oxide of lead acquires a gray colour, dissolves, and forms a kind of plaster with this substance.

Boiling alcohol, of forty degrees strength, dissolves birdlime: the solution is clear, transparent, and of a yellow colour, but as it cools it becomes muddy.

By the filter we can separate from the solution a yellow matter which softens more readily than entire birdlime, and which melts with a gentle heat; diffusing an odour very similar to that of wax, which indeed it resembles in all its characters.

The filtered liquor is bitter, nauseous, and acid; it lets fall a precipitate by water, and leaves on evaporation a substance similar to resin.

Sulphuric ether may be regarded as the true solvent of birdlime: it acts upon this substance readily, divides, and in the end dissolves it almost entirely, leaving a little vegetable matter. The liquor assumes a greenish yellow colour, and reddens strongly the tincture of turnsole. By the addition of a small quantity of water it becomes muddy, and the ether rises to the surface; but if the water be added in sufficient quantity to dissolve the ether, a layer of oil exactly similar to the oil of linseed forms on the surface: with the semi-vitreous oxide of lead this last forms a plaster.

The ethereal solution yields, by evaporation, a yellow substance, which is greasy and soft like wax.

Conclusion.

From what has been now stated it must be very apparent that little analogy exists between birdlime and glutinous matter.

A simple recapitulation will suffice to point out the proper place which this substance ought to occupy among the products of vegetables.

Birdlime is viscid and elastic: it dries slightly by exposure to the air, and becomes brown, but never acquires the brittleness and unalterable properties of glue.

It melts in the fire, inflames, swells up, and burns with a bright flame; but does not diffuse that animal odour which gluten is observed to do.

Water

Water does not dissolve birdlime, but merely takes up the mucilage, extractive matter, and acetic acid, which this substance contains.

Alkalies dissolve it, and when concentrated they convert it into a true soap.

Weak acids soften, and in part dissolve birdlime.

The concentrated sulphuric acid blackens and chars it.

Nitric acid communicates to it a yellow colour, and converts it partly into the oxalic and malic acids, and partly into resin and wax.

The oxymuriatic acid renders it white and solid, forming oxygenated birdlime.

Alcohol has little action on birdlime; it merely dissolves the resin and takes up the acid.

Lastly, Sulphuric ether dissolves it entirely.

Birdlime, therefore, differs from glue in the following particulars:

1. It contains a quantity of free acetous acid.
2. It has very little of an animal nature.
3. Both mucilage and extractive matter may be obtained from it.
4. By the action of the nitric acid it yields a large quantity of resin.
5. It is soluble in ether.

XXIII. *On a new Varnish for Wood. By M.* Parmentier *.

The apothecary of the French military hospital at Gênes, M. Bompoix, has sent me some coffee-cups, the chief merit of which appeared at first to arise from their lightness, but afterwards I discovered that they were still far superior on account of the varnish which covered them. This varnish enjoys a great reputation, and the composition of it is kept a profound secret in that country; I therefore charged M. Bompoix to use every exertion to discover the recipe from which it is made; and he at last obtained it by means of one of his pupils, whose intimacy with the master of the

* From *Annales de Chimie*, tome lvi.

manufactory

manufactory procured him the following recipe, and an article was produced by the use of it equal in quality to the original:

Take of linseed oil one pound and a half.
Amber, one pound.
Pulverized litharge, five ounces.
Pulverized minium (red lead) five ounces.
Pulverized white lead, five ounces.

Boil the linseed oil in an unglazed vessel; make a bag of linen in which the litharge, minium, and white lead may be contained; and suspend it, with its contents, in the vessel; taking care not to allow it to touch the bottom. Continue the ebullition until the oil begins to become brown; then take out the bag with its ingredients and continue to boil the oil, adding a clove of clean garlic; and when this is dried up put in another, and so on to the number of six or seven.

Then melt the amber in an unglazed earthen vessel in the following manner, and when melted pour it into the prepared linseed oil.

Manner of melting the Amber.

Take about two ounces of the linseed oil and add it to the amber, and facilitate its melting by a strong fire: when it is melted mix it with the rest of the linseed oil and boil the whole two minutes; then remove it and strain it through a fine linen cloth; and when it is cold put it into a bottle and stop it well, in order to prevent it from drying up.

Manner of using it.

Take the article which you want to varnish, and polish it well before applying the varnish, which is to be done in the following manner:

Take lamp black, the varnish thus prepared, and a little essence of turpentine; mix them together, and with a pencil lay a coating upon the piece which is to be varnished; when that coat is dry lay on others to the number of four; and when these are dry also, place the article in a stove or furnace,

nace, in order to dry it entirely, and afterwards polish it with powdered pumice-stone and Tripoli.

Manner of preparing the Article which is to be varnished.

It is necessary to make use of walnut-tree, ash, or cherry-tree wood, because these woods are porous, and when they are perfectly dry they will turn better in the lathe; when the article is shaped to your liking you must put it into a stove to dry, after which work it and polish it as if it was to be completely finished, then apply the varnish in the manner above described.

If it is wanted to give the dish a red colour, a little minium, or rather cinnabar, may be put into the varnish; and the same may be done with any other colour you wish to give to the article varnished.

XXIV. *Account of a Series of Experiments, showing the Effects of Compression in modifying the Action of Heat.* By Sir James Hall, *Bart. F.R.S. Edin.**

I. *Antient Revolutions of the Mineral Kingdom.—Vain Attempts to explain them.—Dependence of Geology on Chemistry.—Importance of the Carbonate of Lime.—Dr. Black's Discovery of Carbonic Acid subverted the former Theories depending on Fire, but gave Birth to that of Dr. Hutton.—Progress of the Author's Ideas with regard to that Theory.—Experiments with Heat and Compression, suggested to Dr. Hutton in* 1790.—*Undertaken by the Author in* 1798.—*Speculations on which his Hopes of Success were founded.*

Whoever has attended to the structure of rocks and mountains, must be convinced that our globe has not always existed in its present state; but that every part of its mass, so far at least as our observations reach, has been agitated and subverted by the most violent revolutions.

* From *Transactions of the Royal Society of Edinburgh*, vol. vii.

Facts

Facts leading to such striking conclusions, however imperfectly observed, could not fail to awaken curiosity, and give rise to a desire of tracing the history, and of investigating the causes, of such stupendous events; and various attempts were made in this way, but with little success; for, while discoveries of the utmost importance and accuracy were made in astronomy and natural philosophy, the systems produced by the geologists were so fanciful and puerile, as scarcely to deserve a serious refutation.

One principal cause of this failure seems to have lain in the very imperfect state of chemistry, which has only of late years begun to deserve the name of a science. While chemistry was in its infancy, it was impossible that geology should make any progress; since several of the most important circumstances to be accounted for by this latter science are admitted on all hands to depend upon principles of the former. The consolidation of loose sand into strata of solid rock; the crystalline arrangement of substances accompanying those strata, and blended with them in various modes; are circumstances of a chemical nature, which all those who have attempted to frame theories of the earth have endeavoured by chemical reasonings to reconcile to their hypotheses.

Fire and water, the only agents in nature by which stony substances are produced, under our observation, were employed by contending sects of geologists to explain all the phænomena of the mineral kingdom.

But the known properties of water are quite repugnant to the belief of its universal influence, since a very great proportion of the substances under consideration are insoluble, or nearly so, in that fluid; and since, if they were all extremely soluble, the quantity of water which is known to exist, or that could possibly exist, in our planet, would be far too small to accomplish the office assigned to it in the Neptunian theory*. On the other hand, the known properties of fire are no less inadequate to the purpose; for various substances which frequently occur in the mineral king-

* Illustrations of the Huttonian Theory, by Mr. Professor Playfair, 480.

dom,

dom, seem, by their presence, to preclude its supposed agency; since experiment shows, that, in our fires, they are totally changed or destroyed.

Under such circumstances the advocates of either element were enabled, very successfully, to refute the opinions of their adversaries, though they could but feebly defend their own: and owing perhaps to this mutual power of attack, and for want of any alternative to which the opinions of men could lean, both systems maintained a certain degree of credit; and writers on geology indulged themselves, with a sort of impunity, in a style of unphilosophical reasoning which would not have been tolerated in other sciences.

Of all mineral substances the carbonate of lime is unquestionably the most important in a general view. As limestone or marble it constitutes a very considerable part of the solid mass of many countries, and in the form of veins and nodules of spar pervades every species of stone. Its history is thus interwoven in such a manner with that of the mineral kingdom at large, that the fate of any geological theory must very much depend upon its successful application to the various conditions of this substance. But till Dr. Black, by his discovery of carbonic acid, explained the chemical nature of the carbonate, no rational theory could be formed of the chemical revolutions which it has undoubtedly undergone.

This discovery was in the first instance hostile to the supposed action of fire; for the decomposition of limestone by fire in every common kiln being thus proved, it seemed absurd to ascribe to that same agent the formation of limestone, or of any mass containing it.

The contemplation of this difficulty led Dr. Hutton to view the action of fire in a manner peculiar to himself, and thus to form a geological theory, by which, in my opinion, he has furnished the world with the true solution of one of the most interesting problems that has ever engaged the attention of men of science.

He supposed,

I. That heat has acted, at some remote period, on all rocks.

II. That

II. That during the action of heat all these rocks (even such as now appear at the surface) lay covered by a superincumbent mass, of great weight and strength.

III. That in consequence of the combined action of heat and pressure, effects were produced different from those of heat on common occasions; in particular, that the carbonate of lime was reduced to a state of fusion, more or less complete, without any calcination.

The essential and characteristic principle of his theory is thus comprised in the word compression; and by one bold hypothesis, founded on this principle, he undertook to meet all the objections to the action of fire, and to account for those circumstances in which minerals are found to differ from the usual products of our furnaces.

This system, however, involves so many suppositions, apparently in contradiction to common experience, which meet us on the very threshold, that most men have hitherto been deterred from the investigation of its principles, and only a few individuals have justly appreciated its merits. It was long before I belonged to the latter class; for I must own that, on reading Dr. Hutton's first geological publication, I was induced to reject his system entirely, and should probably have continued still to do so, with the great majority of the world, but for my habits of intimacy with the author; the vivacity and perspicuity of whose conversation formed a striking contrast to the obscurity of his writings. I was induced by that charm, and by the numerous original facts which his system had led him to observe, to listen to his arguments in favour of opinions which I then looked upon as visionary. I thus derived from his conversation the same advantage which the world has lately done from the publication of Mr. Playfair's Illustrations; and experienced the same influence which is now exerted by that work on the minds of our most eminent men of science.

After three years of almost daily warfare with Dr. Hutton, on the subject of his theory, I began to view his fundamental principles with less and less repugnance. There is a period, I believe, in all scientific investigations, when the conjectures of genius cease to appear extravagant, and when

we balance the fertility of a principle, in explaining the phænomena of nature, against its improbability as an hypothesis: the partial view which we then obtain of truth is perhaps the most attractive of any, and most powerfully stimulates the exertions of an active mind. The mist which obscured some objects dissipates by degrees, and allows them to appear in their true colours; at the same time, a distant prospect opens to our view, of scenes unsuspected before.

Entering now seriously into the train of reasoning followed by Dr. Hutton, I conceived that the chemical effects ascribed by him to compression, ought, in the first place, to be investigated; for unless some good reason were given us for believing that heat would be modified by pressure, in the manner alleged, it would avail us little to know that they had acted together. He rested his belief of this influence on analogy, and on the satisfactory solution of all the phænomena furnished by this supposition. It occurred to me, however, that this principle was susceptible of being established in a direct manner by experiment, and I urged him to make the attempt; but he always rejected this proposal, on account of the immensity of the natural agents, whose operations he supposed to lie far beyond the reach of our imitation; and he seemed to imagine that any such attempt must undoubtedly fail, and thus throw discredit on opinions already sufficiently established, as he conceived, on other principles. I was far, however, from being convinced by these arguments; for, without being able to prove that any artificial compression to which we could expose the carbonate would effectually prevent its calcination in our fires, I maintained, that we had as little proof of the contrary, and that the application of a moderate force might possibly perform all that was hypothetically assumed in the Huttonian theory. On the other hand, I considered myself as bound in practice to pay deference to his opinion, in a field which he had already so nobly occupied, and abstained, during the remainder of his life, from the prosecution of some experiments with compression which I had begun in 1790.

In 1798 I resumed the subject with eagerness, being still

of

of opinion that the chemical law which forms the basis of the Huttonian theory ought, in the first place, to be investigated experimentally; all my subsequent reflections and observations having tended to confirm my idea of the importance of this pursuit, without in any degree rendering me more apprehensive as to the result.

In the arrangement of the following paper I shall first confine myself to the investigation of the chemical effects of heat and compression, reserving to the concluding part the application of my results to geology. I shall then appeal to the volcanos, and shall endeavour to vindicate the laws of action assumed in the Huttonian theory, by showing that lavas, previous to their eruptions, are subject to similar laws; and that the volcanos, by their subterranean and submarine exertions, must produce, in our times, results similar to those ascribed, in that theory, to the former action of fire.

In comparing the Huttonian operations with those of the volcanos, I shall avail myself of some facts, brought to light in the course of the following investigations, by which a precise limit is assigned to the intensity of the heat, and to the force of compression, required to fulfil the conditions of Dr. Hutton's hypothesis: for according to him the power of those agents was very great, but quite indefinite; it was therefore impossible to compare their supposed effects in any precise manner with the phænomena of nature.

My attention was almost exclusively confined to the carbonate of lime, about which I reasoned as follows: the carbonic acid, when uncombined with any other substance, exists naturally in a gaseous form at the common temperature of our atmosphere; but when in union with lime, its volatility is repressed, in that same temperature, by the chemical force of the earthy substance which retains it in a solid form. When the temperature is raised to a full red-heat, the acid acquires a volatility by which that force is overcome, it escapes from the lime, and assumes its gaseous form. It is evident, that were the attractive force of the lime increased, or the volatility of the acid diminished by

any means, the compound would be enabled to bear a higher heat without decomposition than it can in the present state of things. Now pressure must produce an effect of this kind; for when a mechanical force opposes the expansion of the acid, its volatility must, to a certain degree, be diminished. Under pressure, then, the carbonate may be expected to remain unchanged in a heat by which, in the open air, it would have been calcined. But experiment alone can teach us what compressing force is requisite to enable it to resist any given elevation of temperature, and what is to be the result of such an operation. Some of the compounds of lime with acids are fusible, others refractory; the carbonate, when constrained by pressure to endure a proper heat, may be as fusible as the muriate.

One circumstance, derived from the Huttonian theory, induced me to hope that the carbonate was easily fusible, and indicated a precise point under which that fusion ought to be expected. Nothing is more common than to meet with nodules of calcareous spar inclosed in whinstone; and we suppose, according to the Huttonian theory, that the whin and the spar had been liquid together, the two fluids keeping separate like oil and water. It is natural, at the junction of these two, to look for indications of their relative fusibilities; and we find, accordingly, that the termination of the spar is generally globular and smooth; which seems to prove that, when the whin became solid, the spar was still in a liquid state; for had the spar congealed first, the tendency which it shows, on all occasions of freedom, to shoot out into prominent crystals, would have made it dart into the liquid whin, according to the peculiar forms of its crystallization, as has happened with the various substances contained in whin, much more refractory than itself, namely, augite, felspar, &c., all of which having congealed in the liquid whin, have assumed their peculiar forms with perfect regularity. From this I concluded, that when the whin congealed, which must have happened about 28° or 30° of Wedgwood, the spar was still liquid. I therefore expected, if I could compel the carbonate to bear a heat of

28°

28° without decomposition, that it would enter into fusion. The sequel will show that this conjecture was not without foundation.

I shall now enter upon the description of those experiments, the result of which I had the honour to lay before this society on the 30th of August last (1804); fully aware how difficult it is, in giving an account of above five hundred experiments, all tending to one point, but differing much from each other in various particulars, to steer between the opposite faults of prolixity and barrenness. My object shall be to describe, as shortly as possible, all the methods followed, so as to enable any chemist to repeat the experiments; and to dwell particularly on such circumstances only as seem to lead to conclusions of importance.

The result being already known, I consider the account I am about to give of the execution of these experiments, as addressed to those who take a particular interest in the progress of chemical operations:—in the eyes of such gentlemen I trust that none of the details into which I must enter will appear superfluous.

II. *Principle of Execution upon which the following Experiments were conducted.—Experiments with Gun-Barrels filled with baked Clay, and welded at the Muzzle.—Method with the Fusible Metal.—Remarkable Effects of its Expansion.—Necessity of introducing Air.—Results obtained.*

When I first undertook to make experiments with heat acting under compression, I employed myself in contriving various devices of screws, of bolts, and of lids, so adjusted, I hoped, as to confine all elastic substances; and perhaps some of them might have answered. But I laid aside all such devices, in favour of one which occurred to me in January 1798; which, by its simplicity, was of easy application in all cases, and accomplished all that could be done by any device, since it secured perfect strength and tightness to the utmost that the vessels employed could bear, whether formed of metallic or earthy substance. The device

depends upon the following general view: If we take a hollow tube or barrel (AD, fig. 1. Plate IV.) closed at one end and open at the other, of one foot or more in length, it is evident, that by introducing one end into a furnace, we can apply to it as great heat as art can produce, while the other end is kept cool, or, if necessary, exposed to extreme cold. If then the substance which we mean to subject to the combined action of heat and pressure be introduced into the breech or closed end of the barrel (CD), and if the middle part be filled with some refractory substance, leaving a small empty space at the muzzle (AB), we can apply heat to the muzzle, while the breech containing the subject of experiment is kept cool, and thus close the barrel by any of the numerous modes which heat affords, from the welding of iron to the melting of sealing-wax. Things being then reversed, and the breech put into the furnace, a heat of any required intensity may be applied to the subject of experiment, now in a state of constraint.

My first application of this scheme was carried on with a common gun-barrel, cut off at the touch-hole, and welded very strongly at the breech by means of a plug of iron. Into it I introduced the carbonate, previously rammed into a cartridge of paper or pasteboard, in order to protect it from the iron, by which, in some former trials, the subject of experiment had been contaminated throughout during the action of heat. I then rammed the rest of the barrel full of pounded clay, previously baked in a strong heat, and I had the muzzle closed, like the breech, by a plug of iron welded upon it in a common forge; the rest of the barrel being kept cold during this operation by means of wet cloths. The breech of the barrel was then introduced horizontally into a common muffle, heated to about 25° of Wedgwood. To the muzzle a rope was fixed in such a manner that the barrel could be withdrawn without danger from an explosion*. I likewise, about this time, closed the muzzle of the

* On one occasion the importance of this precaution was strongly felt. Having inadvertently introduced a considerable quantity of moisture into a welded barrel, an explosion took place, before the heat had risen to redness, by

the barrel by means of a plug fixed by solder only; which method had this peculiar advantage, that I could shut and open the barrel without having recourse to a workman. In these trials, though many barrels yielded to the expansive force, others resisted it, and afforded some results that were in the highest degree encouraging, and even satisfactory, could they have been obtained with certainty on repetition of the process. In many of them chalk, or common limestone previously pulverised, was agglutinated into a stony mass, which required a smart blow of a hammer to break it, and felt under the knife like a common limestone; at the same time the substance, when thrown into nitric acid, dissolved entirely with violent effervescence.

In one of these experiments, owing to the action of heat on the cartridge of paper, the baked clay, which had been used to fill the barrel, was stained black throughout, to the distance of two-thirds of the length of the barrel from its breech. This circumstance is of importance, by showing, that though all is tight at the muzzle, a protrusion may take place along the barrel, greatly to the detriment of complete compression: and, at the same time, it illustrates what has happened occasionally in nature, where the bituminous matter seems to have been driven by superior local heat from one part of a coaly bed, though retained in others, under the same compression; the bitumen so driven off being found, in other cases, to pervade and tinge beds of slate and of sandstone.

I was employed in this pursuit in spring 1800, when an event of importance interrupted my experiments for about a year. But I resumed them in March 1801, with many new

by which part of the barrel was spread out to a flat plate, and the furnace was blown to pieces. Dr. Kennedy, who happened to be present on this occasion, observed, that notwithstanding this accident, the time might come when we should employ water in these experiments to assist the force of compression. I have since made great use of this valuable suggestion: but he scarcely lived, alas! to see its application; for my first success in this way took place during his last illness.—I have been exposed to no risk in any other experiment with iron barrels; matters being so arranged that the strain against them has only commenced in a red heat, in which the metal has been so far softened as to yield by laceration like a piece of leather.

plans of execution, and with considerable addition to my apparatus.

In the course of my first trials, the following mode of execution had occurred to me, which I now began to put in practice. It is well known to chemists that a certain composition of different metals* produces a substance so fusible as to melt in the heat of boiling-water. I conceived that great advantage, both in point of accuracy and dispatch, might be gained in these experiments, by substituting this metal for the baked clay above mentioned: that after introducing the carbonate into the breech of the barrel, the fusible metal, in a liquid state, might be poured in so as to fill the barrel to its brim: that when the metal had cooled and become solid, the breech might, as before, be introduced into a muffle, and exposed to any required heat, while the muzzle was carefully kept cold. In this manner, no part of the fusible metal being melted but what lay at the breech, the rest, continuing in a solid state, would effectually confine the carbonic acid: that after the action of strong heat had ceased, and after all had been allowed to cool completely, the fusible metal might be removed entirely from the barrel, by means of a heat little above that of boiling-water, and far too low to occasion any decomposition of the carbonate by calcination, though acting upon it in freedom; and then that the subject of experiment might as before be taken out of the barrel.

This scheme, with various modifications and additions which practice has suggested, forms the basis of most of the following methods.

In the first trial a striking phænomenon occurred, which gave rise to the most important of these modifications. Having filled a gun-barrel with the fusible metal without any carbonate, and having placed the breech in a muffle, I was surprised to see, as the heat approached to redness, the liquid metal exuding through the iron in innumerable minute drops dispersed all round the barrel. As the heat advanced this exudation increased, till at last the metal flowed out in continued streams, and the barrel was quite destroyed.

* Eight parts of bismuth, five of lead, and three of tin.

On

On several occasions of the same kind, the fusible metal, being forced through some very minute aperture in the barrel, spouted from it to the distance of several yards, depositing upon any substance opposed to the stream a beautiful assemblage of fine wire exactly in the form of wool. I immediately understood that the phænomenon was produced by the superior expansion of the liquid over the solid metal, in consequence of which the fusible metal was driven through the iron as water was driven through silver* by mechanical percussion in the Florentine experiment. It occurred to me that this might be prevented by confining along with the fusible metal a small quantity of air, which, by yielding a little to the expansion of the liquid, would save the barrel. This remedy was found to answer completely, and was applied in all the experiments made at this time†.

I now proposed, in order to keep the carbonate clean, to inclose it in a small vessel; and to obviate the difficulty of removing the result at the conclusion of the experiment, I further proposed to connect that vessel with an iron ramrod,

* Essays of Natural Experiments made in the Academie del Cimento, translated by Waller, London, 1684, page 117. The same in Musschenbroek's Latin translation, Lugd. Bat. 1731. p. 63.

† I found it a matter of much difficulty to ascertain the proper quantity of air which ought to be thus inclosed. When the quantity was too great, the result was injured by diminution of elasticity, as I shall have occasion fully to show hereafter. When too small, or when by any accident the whole of this included air was allowed to escape, the barrel was destroyed.

I hoped to ascertain the bulk of air necessary to give liberty to the expansion of the liquid metal, by measuring the actual quantity expelled by known heats from an open barrel filled with it. But I was surprised to find that the quantity thus discharged exceeded in bulk that of the air which, in the same heats, I had confined along with the carbonate and fusible metal in many successful experiments. As the expansion of the liquid does not seem capable of sensible diminution by an opposing force, this fact can only be accounted for by a distention of the barrel. In these experiments then the expansive force of the carbonic acid, of the included air, and of the fusible metal, acted in combination against the barrel, and were yielded to in part by the distension of the barrel, and by the condensation of the included air. My object was to increase the force of this mutual action, by diminishing the quantity of air, and by other devices to be mentioned hereafter. Where so many forces were concerned, the laws of whose variations were unknown, much precision could not be expected, nor is it wonderful that, in attempting to carry the compressing force to the utmost, I should have destroyed barrels innumerable.

longer than the barrel, by which it could be introduced or withdrawn at pleasure.

A small tube of glass*, or of Reaumur's porcelain, about a quarter of an inch in diameter, and one or two inches in length (fig. 2. A), was half filled with pounded carbonate of lime, rammed as hard as possible; the other half of the tube being filled with pounded silex, or with whatever occurred as most likely to prevent the intrusion of the fusible metal in its liquid and penetrating state. This tube so filled was placed in a frame or cradle of iron (*d f k h*, figs. 3, 4, 5, and 6,) fixed to the end (*m*) of a ramrod (*m n*). The cradle was from six to three inches in length, and as much in diameter as a gun-barrel would admit with ease. It was composed of two circular plates of iron (*d e f g* and *h i k l*, seen edgewise in the figures), placed at right angles to the ramrod, one of these plates (*d e f h*) being fixed to it by the centre (*m*). These plates were connected together by four ribs or flattened wires of iron (*d h*, *e i*, *f k*, and *g l*), which formed the cradle into which the tube (A), containing the carbonate, was introduced by thrusting the adjacent ribs asunder. Along with the tube just mentioned, was introduced another tube (B) of iron or porcelain, filled only with air. Likewise, in the cradle, a pyrometer† piece (C) was

* I have since constantly used tubes of common porcelain, finding glass much too fusible for this purpose.

† The pyrometer-pieces used in these experiments were made under my own eye. Necessity compelled me to undertake this laborious and difficult work, in which I have already so far succeeded as to obtain a set of pieces which, though far from complete, answer my purpose tolerably well. I had lately an opportunity of comparing my set with that of Mr. Wedgwood, at various temperatures, in furnaces of great size and steadiness. The result has proved that my pieces agree as well with each other as his, though with my set each temperature is indicated by a different degree of the scale. I have thus been enabled to construct a table, by which my observations have been corrected, so that the temperatures mentioned in this paper are such as would have been indicated by Mr. Wedgwood's pieces. By Mr. Wedgwood's pieces, I mean those of the only set which has been sold to the public, and by which the melting heat of pure silver is indicated at the 22d degree. I am well aware that the late Mr. Wedgwood, in his Table of Fusibilities, has stated that fusion as taking place at the 28th degree; but I am convinced that his observations must have been made with some set different from that which was afterwards sold.

placed

placed in contact with (A) the tube containing the carbonate. These articles generally occupied the whole cradle; when any space remained it was filled up by a piece of chalk dressed for the purpose. (Fig. 4. represents the cradle filled, as just described.)

Things being thus prepared, the gun-barrel, placed erect with its muzzle upwards, was half filled with the liquid fusible metal. The cradle was then introduced into the barrel, and plunged to the bottom of the liquid, so that the carbonate was placed very near the breech (as represented in fig. 5, the fusible metal standing at *o*). The air-tube (B) being placed so as to enter the liquid with its muzzle downwards, retained great part of the air it originally contained, though some of it might be driven off by the heat, so as to escape through the liquid. The metal being now allowed to cool, and to fix round the cradle and ramrod, the air remaining in the air-tube was effectually confined, and all was held fast. The barrel being then filled to the brim with fusible metal, the apparatus was ready for the application of heat to the breech (as shown in fig. 6).

In the experiments made at this time, I used a square brick furnace (figs. 7 and 8), having a muffle (*r s*) traversing it horizontally and open at both ends. This muffle being supported in the middle by a very slender prop, was exposed to fire from below, as well as all round. The barrel was placed in the muffle, with its breech in the hottest part, and the end next the muzzle projecting beyond the furnace, and surrounded with cloths which were drenched with water from time to time. (This arrangement is shown in fig. 7.) In this situation the fusible metal surrounding the cradle being melted, the air contained in the air-tube would of course seek the highest position, and its first place in the air-tube would be occupied by fusible metal. (In fig. 6., the new position of the air is shown at *p q*.)

At the conclusion of the experiment the metal was generally removed by placing the barrel in the transverse muffle, with its muzzle pointing a little downwards, and so that the heat was applied first to the muzzle, and then to the rest of the

the barrel in succession. (This operation is shown in fig. 8.) In some of the first of these experiments I loosened the cradle by plunging the barrel into heated brine, or a strong solution of muriate of lime, which last bears a temperature of 250° of Fahrenheit before it boils. For this purpose I used a pan three inches in diameter, and three feet deep, having a flat bason at top to receive the liquid when it boiled over. The method answered, but was troublesome, and I laid it aside. I have had occasion lately, however, to resume it in some experiments in which it was of consequence to open the barrel with the least possible heat *.

By these methods I made a great number of experiments, with results that were highly interesting in that stage of the business, though their importance is so much diminished by the subsequent progress of the investigation, that I think it proper to mention but very few of them.

On the 31st of March 1801 I rammed forty grains of pounded chalk into a tube of green bottle-glass, and placed it in the cradle as above described. A pyrometer in the muffle along with the barrel indicated 33°. The barrel was exposed to heat during seventeen or eighteen minutes. On withdrawing the cradle, the carbonate was found in one solid mass, which had visibly shrunk in bulk, the space thus left within the tube being accurately filled with metal, which plated the carbonate all over without penetrating it in the least, so that the metal was easily removed. The weight was reduced from forty to thirty-six grains. The substance was very hard, and resisted the knife better than any result of the kind previously obtained; its fracture was crystalline, bearing a resemblance to white saline marble; and its thin edges had a decided semitransparency, a circumstance first observed in this result.

On the 3d of March of the same year I made a similar

* In many of the following experiments lead was used in place of the fusible metal, and often with success; but I lost many good results in this way: for the heat required to liquefy the lead, approaches so near to redness, that it is difficult to disengage the cradle without applying a temperature by which the carbonate is injured. I have found it answer well, to surround the cradle and a few inches of the rod with fusible metal, and to fill the rest of the barrel with lead.

experiment, in which a pyrometer-piece was placed within the barrel, and another in the muffle; they agreed in indicating 23°. The inner tube, which was of Reaumur's porcelain, contained eighty grains of pounded chalk. The carbonate was found, after the experiment, to have lost 3½ grains. A thin rim, less than the 20th of an inch in thickness, of whitish matter, appeared on the outside of the mass. In other respects the carbonate was in a very perfect state; it was of a yellowish colour, and had a decided semi-transparency and saline fracture. But what renders this result of the greatest value is, that on breaking the mass a space of more than the tenth of an inch square was found to be completely crystallized, having acquired the rhomboidal fracture of calcareous spar. It was white and opake, and presented to the view three sets of parallel plates which are seen under three different angles. This substance, owing to partial calcination and subsequent absorption of moisture, had lost all appearance of its remarkable properties in some weeks after its production; but this appearance has since been restored by a fresh fracture, and the specimen is now well preserved by being hermetically inclosed.

[To be continued.]

XXV. *Additional Experiments and Remarks on an artificial Substance which possesses the principal characteristic Properties of Tannin. By* CHARLES HATCHETT, *Esq. F. R. S.*

[Concluded from p. 68.]

§ VII.

FROM the experiments which have been related, it appears, that three varieties of the artificial tanning substance may be formed, viz.

1st, That which is produced by the action of nitric acid upon any carbonaceous substance, whether vegetable, animal, or mineral.

2dly, That which is formed by distilling nitric acid from common

common resin, indigo, dragon's blood, and various other substances: and,

3dly, That which is yielded to alcohol by common resin, elemi, asa fœtida, camphor, &c., after these bodies have been for some time previously digested with sulphuric acid.

Upon these three products I shall now make a few remarks, which I have hitherto postponed, in order that the account of the experiments might not be interrupted.

The first variety is that which is the most easily formed; and from some experiments which were purposely made, I find that 100 grains of dry vegetable charcoal afford 120 of the tanning substance; but, as it is extremely difficult completely to expel moisture, or even the whole of the nitric acid which has been employed*, an allowance of about three or four grains ought to be made; so that after this deduction we may conclude, that 100 grains of vegetable charcoal yield 116 or 117 of the dry tanning substance.

The proportions of the constituent parts of this substance I have not as yet ascertained; but, from the manner by which it is produced, carbon is evidently the base of it, and is the predominating essential ingredient.

From § III. experiment F. it also appears, that the other component parts are oxygen, hydrogen, and nitrogen; for, when the artificial tanning substance was distilled, ammonia and carbonic acid were obtained, exclusive of a very small portion of a yellow liquor, which stained the upper part of the retort, and which, from its tenacity and insolubility in water and alcohol, appeared to be of an oily nature.

As I had taken every precaution respecting the charcoal which had been employed, I was at first induced to consider the above facts as almost positively demonstrative of the presence of hydrogen in charcoal; but upon further reflection, and upon weighing some of the circumstances which attend the formation of the artificial tanning substance, I still feel on this point very considerable doubt; for I have constantly

* The most effectual method of expelling the nitric acid is, to reduce the tanning substance to powder, and repeatedly evaporate different portions of distilled water from it in a glass or porcelain basin.

observed,

observed, that diluted nitric acid acts upon charcoal more effectually in the formation of the tanning substance than when it is employed in a concentrated state; and it appears therefore very probable that hydrogen may have been afforded by a portion of water decomposed during the process. For, admitting that the new compound (formed by the action of nitric acid upon charcoal) may possess a certain degree of affinity for hydrogen, this, being exerted simultaneously with the affinity for oxygen possessed by nitrous gas, may (especially when the last is in a nascent state) effect a decomposition of a portion of water, the hydrogen of which would therefore enter into the composition of the tanning substance, whilst the oxygen would supply the place of part of that which had been taken from the nitric acid.

Many of the properties of the tanning substance prepared from coal by nitric acid are very remarkable, particularly those which have been noticed in § III. experiments F and G; for surely it is not a little singular, that this substance when burned should emit an odour so very similar to animal matter, notwithstanding that the tanning substance had been prepared from pure vegetable charcoal. And again, in experiment G, the portion which had not been precipitated by solution of isinglass was, when dried, found to possess a strong vegetable odour very analogous to oak bark, although charcoal is inodorous, and isinglass very nearly so.

But, after all, the most extraordinary properties of this substance are certainly those which so nearly approach it to the vegetable principle called *tannin*; for it perfectly resembles this principle by its solubility in water and in alcohol, by its action upon gelatine and upon skin, by the effects which it produces upon metallic solutions, upon those of the earths, and of the alkalis.

The sulphuric and muriatic acids also affect the solutions of it as they do those of tannin; and the only marked difference which as yet has been found in the characters of the artificial substance and of tannin is, that the former is produced, whilst the varieties of the latter are more or less destroyed by nitric acid. This, for the present at least, must draw a line of separation between them; but we must not forget,

forget, that even the varieties of tannin * do not accord in the degree of destructibility.

The second variety of the tanning substance is obtained from a great number of vegetable bodies; such as indigo, dragon's blood, common resin, &c. &c., by digesting and distilling them with nitric acid. It is not, therefore, quite so readily prepared as that which was first described, and its relative quantity, when compared with that of the substance employed to produce it, is less.

As resin and some of the other bodies do not afford it until they have been repeatedly treated with nitric acid, and as during each operation nitrous gas is produced, whilst the strength of the acid which comes over is diminished, it seems

* I shall here venture to state some ideas which have occurred to me on the probable cause and mode of the formation of tannin.

Mr. Biggin has proved, that similar barks, when taken from trees at different seasons, differ as to the quantities of tannin contained in them.—(Phil. Trans. 1799, p. 259.)

Mr. Davy also observes, "that the proportions of the astringent principles in barks vary considerably according as their age and size are different."

"That in every astringent bark the interior white bark (which is the part next to the alburnum) contains the largest quantity of tannin. The proportion of extractive matter is generally greatest in the middle or coloured part; but the epidermis seldom furnishes either tannin or extractive matter."

Moreover Mr. Davy remarks, "that the white cortical layers are comparatively most abundant in young trees, and hence their barks contain in the same weight a larger proportion of tannin than the barks of old trees."—(Phil. Trans. 1803, p. 264.)

We find, therefore,

1st, That the proportion of tannin in the same trees is different at different seasons.

2dly, That tannin is principally contained in the white cortical layers, or interior white bark, which is next to the alburnum or new wood: and,

3dly, That these white cortical layers are comparatively most abundant in young trees, and that their barks consequently contain in the same weight more tannin than the barks of old trees.

I shall not make any remarks on the first of these facts, as it accords with other similar effects, which are the natural consequences of the processes and periods of vegetation; but the second and third appear to be important; for they prove that tannin is principally formed, or at least deposited, in the interior white bark, which is next to the alburnum or new wood; so that in the very same part where the successive portions of new wood are to be elaborated and deposited, we find the principal portion of tannin.

It should seem, therefore, that there is an intimate connection between the formation

seems almost evident that this tanning substance is formed in consequence of part of the oxygen of the nitric acid becoming combined with the hydrogen of the original body, so as to form water; and the carbon, being thus in some measure denuded, is rendered capable of being gradually acted upon by the nitric acid in a manner nearly similar to that which takes place when it has been previously converted into coal.

The colour of the precipitates which this tanning substance yields with gelatine is constantly pale or deep yellow, whilst that of the precipitates formed by the first variety is always brown; I am therefore induced to believe that the different colours of the precipitates produced by the varieties of tannin depend on the state of their carbon.

When resin and the other bodies were treated as above

formation of new wood and the formation of tannin in such vegetables as afford the latter; and this idea is corroborated when the chemical nature of these substances is considered.

From experiments made on the ligneous substance of vegetables, or the woody fibre, it appears to be composed of carbon, oxygen, hydrogen, and nitrogen; but of these its principal and essential ingredient is carbon.

In like manner carbon is unquestionably the basis and principal ingredient of tannin. Considering, therefore, that both of these substances consist principally of carbon; that tannin is secreted in that part of barks where the formation and deposition of new wood take place; and that the quantity of tannin is the most considerable in young trees, and seems therefore to keep pace with their more vigorous growth, and consequent rapid formation of wood; it appears very probable that those vegetables which contain tannin have the faculty of absorbing more carbon and of the other principles than are immediately required in the formation of the different proximate vegetable substances, especially the woody fibre: that this excess, by chemical combination, becomes tannin, which is secreted in the white interior bark: that in this state it is a principle peculiarly fitted to concur by assimilation to form new wood: that it is therefore subsequently decomposed at the proper period, and is employed in the formation of the new wood: that there is not a continual accumulation of tannin in the vegetables which afford it, as it is successively formed in and with the white cortical layers, and is successively decomposed by concurring to form new wood: and, lastly, that as the vegetable approaches more nearly to the full maturity of its growth, when wood is less rapidly and less plentifully formed, so in like manner less tannin is secreted; for, the fabric being nearly completed, fewer materials are required.

Such I am inclined to suspect, from the facts which have been adduced, to be the cause and mode by which tannin is formed in oaks and other vegetables; but I make this statement only as a probable conjecture, which may be refuted or confirmed by future observations.

described

described with nitric acid, the quantity obtained of the tanning substance was much less than when an equal quantity of coal was employed, or even when these bodies had been previously converted into coal in the humid way by sulphuric acid.

The cause of this seems to be, that a number of other products are simultaneously formed, all of which require more or less of carbon as a constituent ingredient, so that, in consequence of the affinities which prevail under the existing circumstances, some bodies by treatment with nitric acid afford but little, and others none, of the tanning substance.

The greatest proportion of this substance was yielded by indigo, common resin, and stick lac.

The quantity obtained from asa fœtida and gum ammoniac was less.

Benzoin, balsam of Tolu, balsam of Peru, and dragon's blood, were inferior to the former in this respect; so that the development, or rather production of benzoic acid *, appeared

* The expression "production of benzoic acid" may appear objectionable, and I shall therefore take this opportunity so observe, that I much suspect the present established opinion respecting the balsams and benzoic acid to be erroneous: for the balsams are defined as bodies composed of resin and benzoic acid; consequently the latter, when obtained in a separate state, is considered as an original ingredient or educt.

I am, however, inclined to a contrary opinion; for I consider the balsams as peculiar substances, which, although nearly approaching to the nature of resins, are nevertheless different in respect to the original combination of their elementary principles; which combination, however, is with much facility modified by various causes, and especially by a certain increase of temperature, so that a new arrangement of the elementary principles takes place, part being formed into resin, and part into benzoic acid.

Many facts appear more or less to support this opinion; for whether benzoic acid is obtained by simple sublimation, or by merely digesting benzoin in boiling water, according to Geoffroy's method, or by the addition of lime, as recommended by Scheele, or by employing alkalis in a similar manner, nothing positive can be inferred from any of these operations to prove that benzoic acid is obtained as an educt, but rather the contrary, when we reflect on the affinities which are most likely to prevail under the circumstances of the different processes, and on the variable proportions of the benzoic acid; and although benzoic acid has been discovered in the urine of infants, in that of many adults, and constantly in that of graminivorous quadrupeds, such

peared partly to counteract the formation of the tanning substance; but oxalic acid, when formed in any considerable quantity, seemed absolutely to prevent the formation of this substance; for whilst abundance of the former was obtained from gum arabic, tragacanth, manna, and guaiacum, not any of the latter could be produced.

Common liquorice appears at first to be an exception; but from the smallness of the quantity, and the colour of the precipitate which it produced with solution of isinglass, I am almost convinced that the tanning substance was formed by the action of the nitric acid on a portion of uncombined carbon, which, being in a state approaching to coal, is probably the cause of the blackness of the common liquorice.

As the formation of the tanning substance has been my

such as the camel, the horse, and the cow, (*Système des Connoissances Chimiques*, par Fourcroy, 4to edit. tom. iv. p. 158;) yet all this certainly appears to be in favour of its being a chemical product.

I have observed, when benzoin, balsam of Tolu, and balsam of Peru, were dissolved in sulphuric acid, that a great quantity of beautifully crystallized white benzoic acid was sublimed during digestion; and as it is produced in so very pure a state by this single and simple operation, I would recommend a trial of the process to those who prepare benzoic acid for commerce; but I am not certain whether this mode may prove more œconomical than those which at present are employed.

When dragon's blood, however, was treated in the same manner with sulphuric acid, I could not obtain a particle of benzoic acid; nor did I succeed much better when I had recourse to lime, according to Scheele's process; for, although a considerable quantity of the substance was thus rendered soluble in water, yet by the addition of muriatic acid I obtained only a slight appearance of benzoic acid, accompanied by a copious precipitate of red resin, notwithstanding that the solution had acquired a powerful and peculiar balsamic odour.

But in a former part of this paper I have stated, that when dragon's blood was dissolved in nitric acid, and afterwards evaporated to dryness, it yielded about 6 per cent. of benzoic acid. Now, if this had been originally present in dragon's blood in the state of benzoic acid, some stronger evidence of it might reasonably have been expected in each process; but this not being the case, I am inclined to consider it as produced, and not educed, by the action of the nitric acid on the original principles of the dragon's blood; and I am also persuaded that similar but more general effects take place when benzoin or any of the balsams are subjected to the different processes by which benzoic acid is obtained; so that to me this last seems to be as much a chemical product as the oxalic, the acetous, and other of the vegetable acids.

The succinic acid also appears to be a product and not an original ingredient of amber.

principal object, I have not thought it necessary to enter at present into too minute a detail of other particulars, and have therefore only thus cursorily noticed some of the principal effects produced by nitric acid on the resins, balsams, &c. Those, however, who are conversant with chemistry, will undoubtedly perceive that these effects deserve to be accurately investigated; and that the resins, balsams, gum resins, and gums, should be regularly examined by every possible method, not merely on account of the individual substances which may become the subjects of experiment, but because there is reason to expect that from such an investigation, medicine, with the arts and manufactures, may derive many advantages, whilst the mysterious processes and effects of vegetation may very probably receive considerable elucidation.

Concerning the third variety of the tanning substance, which is produced by the action of sulphuric acid on the resins, gum resins, &c., I shall here add but little to that which I have already stated in the latter part of the second section of my first paper, and in the account which I have lately given of an experiment on camphor.

This variety appears to be uniformly produced during a certain period of the process, but by a long continuance of the digestion I have reason to believe that it is destroyed.

Substances, such as the gums, which afford much oxalic acid by treatment with other acids, do not apparently yield any of this tanning substance.

The energy of its action on gelatine and skin is certainly inferior to that of the first variety, into which however, as we have seen, it may easily be converted by nitric acid.

From the mode of its formation there does not appear to be any positive evidence that it contains nitrogen like the first and second varieties, and perhaps the absence of nitrogen may be the cause of its less powerful action; this I have not as yet ascertained, but it is my intention more particularly to notice in a future paper the general properties of this substance.

XXVI. *A simple and accurate Mode of constructing Gasometers for Purposes where uniform Pressure is essential, by the Application of the Hydrostatic Regulator. By* Joseph Steevens, *Esq.*

To Mr. Tilloch.

SIR,

I have herewith sent you a drawing and description of a gasometer constructed on the principle of my hydrostatic regulator, described in your twentieth volume, page 289, which, I conceive, will be found fully as useful as the most complex and expensive instrument of this kind that has yet been produced. I have endeavoured to be brief in my description, as the reader will easily comprehend the uses and modes of employing the instrument from the descriptions you have already published of other gasometers, and of the hydrostatic regulator already referred to.—I am, sir,

Your obedient servant,

Garlic Hill,
March 8, 1806.

Joseph Steevens.

A tin vessel, A, (Plate V.) of 12 inches diameter and 12 inches deep, is supported by four pillars *p*, *p*, *p*, *p* fixed at the top into the circular piece of wood *m*, *m*, and at the bottom into the wood base *q q* about an inch and a half thick, and hollowed out in the middle to receive the convex surface of the vessel B, on which it is fixed by means of the loops and hooks *o*, *o*, *o*, *o*: through the base *q q* two holes are made to allow the cocks *g c* to pass, for readily removing B for the purpose of filling, &c., though by having a cock near the bottom it may be filled in its present situation. The vessel B is about 13 inches diameter and 10 deep. *d* is a collar of leathers fixed to the vessel A, through which a metal tube *d e* passes, extending down the tube *f g* to *e*. To the cock *g* a recurved tube is fixed, and so bent that it returns and nearly touches the upper part of the gas-holder B. A slip of metal may be placed diagonally under this tube, to prevent the noise of the water while running from A to B. The vessel A being filled with water, and B with gas, in the

 usual

usual manner, (for B is no more than the common gas-holder,) screw in the collar *d*, shut the cock *c*, and open *g*, the water will run in until the density of the gas in B is capable of resisting its effective pressure, which will always be exactly equal to a column of water whose height is equal to the height of the orifice *e*, of the tube *de*, above that of the recurved tube in the vessel B: for as the air in entering the vessel A has to counteract a column of water equal to the depth *e* below the surface, so will the gas in B be pressed with the whole column, minus that part of it above *e*; for the superincumbent column above *e* has no share in the pressure, which may be increased and decreased at pleasure by elevating and depressing *e*, without regard to the quantity of water in A, and which pressure will continue uniform until the water subsides to the level of the orifice *e*.—See the description of my hydrostatic regulator, Phil. Mag. vol. xx. p. 289.

From the facility with which the pressure in this gasometer may be altered, it becomes a very useful instrument in all cases where the blowpipe is used; whether when charged with oxygen gas for the purpose of deflagrating or of deoxidating metals, the combustion of the diamond, or other experiments where intense heat is required; or whether it is charged with atmospheric air for miniature, glass-blowing, soldering, or the like. It will also be found useful for filling vessels for a variety of experiments in pneumatic chemistry, as well as for playing jets of flame, &c. If a cock, provided with a jet capable of being inclined in any angle to the horizon, be fixed into the tube *fg* near *g*, and the tube *de* so elevated that the orifice *e* arrives near or within the bottom of the vessel A, a useful and accurate apparatus will be formed for illustrating the parabolic theory of projectiles; for, as the pressure and efflux is uniform, the horizontal ranges will be equal at equal angles above and below 45°, and the curve described distinct and well defined. An instrument of this kind has been employed in the Mathematical Society's present course of lectures, and is now in their repository.

XXVII. *Pro-*

XXVII. *Process employed to obtain a Black Liquid, invented by Mr.* CLARKE, *an Englishman, and introduced into Commerce; its Use in marking Linen in a solid and durable Manner, and its Application for printing Cottons or Stuffs. By M.* HERMBSTADT, *of Berlin.*

FOR these two or three years past, a black tincture has been sold for the purpose of marking linen.

A glass polisher and directions for using the tincture accompany the two bottles which contain the ingredients, and the whole is sealed up in a case.

One of the bottles contains the mordant. The other contains the ink, which is of a deep brown colour, and which must be well shaken before making use of it, because it subsides when left to rest.

The part of the linen intended to be marked must be in the first place impregnated with the mordant, which is allowed to dry on the linen. The place which had been wetted is then rubbed with the polisher; an ordinary pen is then dipped in the ink, and the writing is performed on the linen the same as on paper. Neither soap nor any chemical preparation will destroy this writing, which, when well dried, is of a very fine black.

Having chemically analysed these two liquids, I am able to give an account of the ingredients which compose them.

Preparation of the Ink.

Dissolve in nitric acid (aquafortis) what quantity of silver you please. This solution, if the silver has been alloyed with copper, will be of a sapphire blue.

In order to separate the copper from the silver add to the solution twelve times its weight of distilled water, or, for want of it, rain water, and suspend in it a thin plate of copper. In proportion as this plate dissolves, the silver will precipitate itself, perfectly pure, in the form of a white powder. When no more of this powder will precipitate itself, the liquor should be decanted. The powder is then washed in a great quantity of water, until the water thrown upon it

is no longer of a blue cast, but remains perfectly limpid. The residue, *i. e.* this powder, well dried, will be silver in its purest state.

If this residue weighs one ounce, dissolve as much gum senegal and two drachms of white glue in two ounces of distilled water. Mix this solution with three drachms of lamp-black well calcined in a close crucible.

To manufacture this mixture properly, it ought to be triturated in a glass mortar.

This operation being finished, the solution of silver, diluted in eight times its weight of distilled water, is poured upon the above mixture: the whole is then well stirred with a spatula, and the ink is made.

Preparation of the Mordant.

Dissolve two ounces of white glue and as much isinglass, in six ounces of alcohol and as much distilled water. This solution will be made in two days. The B. M. is made use of for the purpose; and care must be taken to stir the two kinds of glue from time to time.

After the whole is dissolved, it must be filtered through flannel, in order to keep back all its mucilaginous particles. The liquid thus filtered, and preserved in a bottle well corked, is then ready for use.

Manner in which the Ink acts.

The solution of silver in the nitric acid is nothing else than the composition of the *lapis infernalis*; and every one knows its properties in staining the skin, nails, &c., of a black colour. If the linen or stuff is first impregnated with the above mordant, which is an animal substance, the ink may be afterwards applied without spreading, and will completely dye every thread of the part to which it is applied, the mordant having previously partly animalized the fibre of the fabric.

Soap, or any other ingredient used in washing, may obliterate the lamp black, but it never takes out the nitrate of silver; and the object proposed is therefore perfectly well attained.

Application

Application of the Ink for printing orange Cotton and other Stuffs.

We may easily conceive that this ink may be employed with advantage for printing cloths of a white, yellow, or rose ground, or any other clear colour.

The cloths or stuffs intended to be printed in this manner, require no other preparation than to be dipped in a solution of parchment or isinglass; and after they are dried they must be rubbed with a glass polisher.

The ink must be thickened for this purpose with a greater quantity of gum senegal, and then applied upon the cloths or stuffs in the usual manner by means of wooden or metal stamps.

Three or four days after this operation the stuffs must be first washed with a great quantity of clear water, and afterwards with soap and water, which will make them appear of a finer black.

XXVIII. *Letter of Messrs.* Civni *and* Petrini *to Professor* Pacchiani, *of Pisa, on the supposed Production of Muriatic Acid by Galvanism* *.

When professor Simon †, by insulating the action of the positive pole of the electrical column under water, obtained oxygen gas and an acid as the result of his analysis; and when he recognised in this acid all the characteristics of the muriatic acid, he supposed that it might be produced by the muscular fibre which he had substituted in the place of the metal wire communicating with the negative pole of the column; and he went no further than imagining that he had only obtained a result new, interesting, and agreeing extremely well with the most luminous facts of pneumatic chemistry.

It was reserved for you to trace this result a little further; and your repetition of the experiment produced this new and

* Abridged from *Annales de Chimie*, tome lvi. p. 269.

† *Annales de Chimie*, tome xli. p. 106.

important truth—"*that hydrogen is susceptible of different degrees of oxygenation.*"

The discovery of the nature of the muriatic acid ought to coincide with that of the different degrees of oxygenation of which hydrogen is susceptible.

"Of all the substances known," says the illustrious Berthollet in his Chemical Statics, "there is none of them, with the exception of hydrogen, which, at an equal weight, can combine with a greater quantity of oxygen, making its characteristic properties disappear, while the substance itself also loses the properties which characterize it. These facts prove that the acid property required to saturate determinate quantities of alkali, is not in proportion to the quantity of oxygen which combines with a base; but that the more it is condensed, the stronger, in consequence, is the action which it evinces; the less it gives of acidity at an equal quantity, because the free acid property which it communicates by its affinity is diminished in consequence of this action. Thus water, exhibiting neither the property of oxygen nor of hydrogen, we may conclude that these two substances are combined at the term where the reciprocal affinity exercises the greatest effect, and when they are in a similar state to that of a neutral salt, in which the acid and alkaline properties are equally become latent; having experienced by their combination a condensation by which their volume is reduced to 0·0002. In the acids the qualities of oxygen prevail; in inflammable liquids those of hydrogen are predominant in such a manner, that in the first combinations the oxygen experiences a degree of saturation less than in water, and in the latter it is the hydrogen which is in this state."

After this opinion of one of the most celebrated chemists of Europe upon the phænomena relative to the combinations of oxygen, we need be no longer surprised to observe hydrogen passing, in your experiments, to a state of acidity, by abandoning a part of the oxygen which saturates it.

We render homage to truth in adopting entirely your ideas upon the decomposition of water by means of the electrical exciter. They coincide too exactly with the luminous principles of the "Chemical Statics" to doubt it for a moment.

Occupied

Occupied in repeating some experiments concerted with you, we were anxious to determine the proportions of the elements of the muriatic acid. We introduced into a glass tube, intended to contain the water which was to pass to the state of muriatic acid, a solution of carbonate of soda or of potash. We closed the upper extremity of the tube, which was traversed by a gold wire communicating with the positive pole of the column by a crooked capillary tube, which entered under a little bell glass filled with a solution of caustic soda, and inverted in a little cup containing the same liquor.

The oxygen, which liberates itself from the water, determines the production of a certain number of particles of muriatic acid, which take the place of a greater or less quantity of particles of carbonic acid in the carbonate of potash or soda according to the amount of their capacity of saturation. The oxygen gas and the carbonic acid gas pass through the tube into the bell glass of the pneumatic apparatus; and this latter, absorbed by the caustic soda, leaves the oxygen gas alone. It is evident that the neutral muriate of soda or of potash contained in the tube which transmits the electrical fluid (and which may be easily separated from the carbonate of soda or of potash that may remain there, by decomposing the latter by the acetic acid, and taking up the acetate by means of alcohol) indicated the quantity of muriatic acid which was there formed, while the air which may pass into the bell glass of the pneumatic apparatus announced the quantity of oxygen which a mass of water, equal to the amount of the weight of the acid formed, and of the oxygen gas obtained, must lose in order to be converted into muriatic acid.

The same results may be obtained by a method much simpler and more rigorous, by substituting in the pneumatic apparatus mercury in the place of caustic soda. In this case, a mixture of carbonic acid and oxygen gas will be obtained in the bell glass of the apparatus; and when the quantities of soda or potash which the carbonic and muriatic acids require for their saturation are exactly known, the quantity of carbonic acid gas liberated necessarily indicates that of the muriatic acid, which has assumed its place in the tube

tube through which the elastic fluid passes to form the muriate of soda or of potash. We have reduced to a very simple formula the general expression of the results of all the experiments of this kind. If, for example, the muriatic acid necessary to neutralise the potash or soda freed from carbonic acid is a quantity expressed by m, and that the oxygen gas obtained is represented by n, $m + n$ will be the weight of the water decomposed, which we express by P: thus if the oxygen contained in P equals $0{\cdot}85\ p$, then the oxygen which contains m is equal to $0{\cdot}85\ p - n$.

XXIX. *On the Decomposition of Water by Galvanism.* By John Cuthbertson, *Esq.*

To Mr. Tilloch.

DEAR SIR,

I had, by some means, overlooked that paragraph in your valuable Magazine (vol. xxiv. p. 285.) wherein you mention you had been informed that I had repeated the experiment on the composition of muriatic acid by Galvanism. I did not know you had mentioned it till I was applied to for such an apparatus as you had mentioned. Your information, however, was not perfectly correct:—I performed the experiment, but did not intend it for publication till I had repeated it in a more accurate manner; I only did it to try what would be the consequence when the experiment was performed in such an incorrect manner as any person might be able to repeat. The water which I used was not distilled; intending, if I found at the termination of the experiment the remaining water to possess any other properties than that which it had before the process, to hold it a sufficient encouragement to undertake an accurate repetition. I found during the process that the gold wire let fall from its surface something that had the appearance of pieces of black rusty metal, and the wire appeared at places much corroded. The platina wire let fall a small white cloud near the end of the process. When one-half of the water had disappeared, the remaining portion, by the usual tests, showed signs of acidity.

dity. Two days before the experiment ended, I placed that end of the tube which had the gold wire at the upper end of the trough, and very soon the platina which had remained bright became black, and, when magnified, had the appearance of black powder covering its surface, from the end to the height of the gold wire. The glass tube was in the form of the sketch sent herewith (Plate V. fig. C.) The lines on the inside represent the two wires; *a b* the gold wire, and *c d* the platina wire.

This result made a repetition necessary in a more accurate manner. Accordingly, I took another clean glass tube bent in the same form, but I substituted, instead of the gold wire *a b*, a platina wire, so that both wires were of the same metal, and introduced distilled water. After this apparatus had stood with its end *a* in connection with the zinc end of the trough three days, the short platina wire assumed the colour of gold, and the long one began to grow black from the lower end to the height of the short wire, and continued so for the space of three weeks. When about one-third of the water had disappeared, I connected the lower end of the tube with the copper end of the trough, and in the space of one day the black powder left the long wire perfectly bright, and the short one became black. In the space of two days that portion of the long wire to the height of the short one obtained a yellowish gold-like tinge. Both remained so for three days; when I placed them in their first situation: the black powder left the short wire, and the long one became black. I immersed in the remaining water a slip of blue test paper; the colour was changed; so that I make no doubt but that, when the operation is continued till half the water disappears, it will give as strong signs of acidity as the former.

You will observe that the ends of the wires are placed parallel to each other; I found that situation to be most favourable to the production of gas: perhaps, if they were placed with their ends opposite each other, the change of colour would not take place.

I am respectfully yours,

JOHN CUTHBERTSON.

No. 64, Poland-street,
March 22, 1806.

XXX. *No-*

XXX. *Notice of Experiments made by the Galvanic Society of Paris on the Discovery announced by M.* PACCHIANI *of the Composition of the Muriatic Acid*.*

As soon as the Galvanic Society was informed that M. Pacchiani announced that he had obtained muriatic acid by taking from water a part of its oxygen, the society's first care was directed to endeavour, as well by means of electricity as of Galvanism, to confirm this discovery so important to science.

M. Pacchiani having solely employed the Galvanic pile, the members determined to employ in their experiments the same agent, in that manner which appeared to them the most convenient and proper; and, above all, which might give results the least susceptible of objections.

First Experiment.

They took a piece of new glass tube of 0·081 parts of a meter in length, and 0·009 parts of a meter of interior diameter. One of the ends of this tube was closed by the lamp; to the other a capillary tube was joined (by fusion), bent so as to come under a bell glass. At the upper part of this tube, and at an equal distance from the junction of the capillary tube, two holes were punctured at the lamp through the solid glass, by means of which apertures there were inserted into the interior of the tube, at a very little distance from its lower extremity, two bits of gold wire of the standard 0·976 purity, and about 0·0005 parts of a meter in diameter, disposed so as not to touch each other, and not to bear against the inside of the tube. These openings were afterwards closed by the lamp. The tube and its capillary addition was filled with pure distilled water. The whole was fixed with bees' wax upon a piece of glass placed upon the middle of a horizontal Galvanic pile of 52 pairs of square plates of 0·108 parts of a meter each side. These plates were separated by bits of leather, the interstices among which were filled up with very pure sand moistened with a solution of muriate of soda. The capillary tube being plunged in a

* From *Annales de Chimie*, tome lvi.

tub

tub of water, its extremity entered below a bell glass filled with the same fluid. The two gold wires being then placed in communication with the two poles of the pile, its activity was immediately manifested by the disengagement of gas in a string of very perceptible bubbles coming from the inferior extremity of each of the gold wires, but in a much greater quantity from that connected with the copper pole. The pile was kept in action, with very little interruption, from the 8th Thermidor to the 11th of the following month. After any interruption whatever, the activity was immediately reproduced by the agitation of the wires communicating with the poles of the pile. It was also remarked that the activity of the pile was constantly stronger from mid-day till four o'clock, when it began to decline. On the 11th Fructidor the apparatus was dismounted, after having been during 34 consecutive days in action, and in an activity of disengagement which may be considered as having been continual. The water was then diminished by one-half its volume. It had lost nothing of its limpidity. The extremities of the gold wire, from which the disengagement of the gas took place in the interior of the tube, were oxidated; the one corresponding with the zinc pole of the pile was most oxidated. The whole of the gas obtained and collected during the experiment was about 793 cubic centimeters. The liquid remaining in the tube was examined with care. It produced no kind of taste upon the tongue, nor any action on tinctures of turnsole and brazil-wood, nor with the solution of nitrate of silver.

The society proceeded afterwards to the trial of the gases disengaged by the action of the pile. After having introduced one measure of it into the eudiometer of Fontana, they made pass into it an equal quantity of nitrous gas made expressly for this experiment. There was an absorption of 77 two hundredth parts upon the volume of the two measures. In order to ascertain if by this absorption all the oxygen the gases contained had entered into combination, a second measure of the same nitrous gas was introduced into the eudiometer after this absorption. It experienced no diminution of volume. They tried to estimate by comparison

the quantity of oxygen which could indicate the absorption produced by the introduction of the first measure of nitrous gas, by trying atmospheric air in the same manner. They consequently introduced one measure into the eudiometer and one of the same nitrous gas. The absorption was 55 two hundredth parts. By considering this absorption as the effect of the combination of the nitrous gas with the quantity of oxygen gas corresponding to 0·22, which atmospheric air contains of it, they concluded that the absorption of the 77 two hundredth parts, produced with the gas of the pile, represented proportionally the combination of the same nitrous gas, with a little less than 0·31 of oxygen. It was then observed that, the measures of the gas having been separately and successively introduced into the eudiometer, it might have happened that they were not intimately enough mixed together, and that, consequently, the absorption might not be complete. It was thought more convenient to make the gases pass at first by separate measures under a bell glass, and afterwards to introduce the whole volume of them into the eudiometer. The preceding experiments having been repeated in this manner, there was, with the gas of the pile and the nitrous gas, an absorption of 92 two hundredth parts in place of 77 resulting from the same trial by the former mode; and with atmospheric air and the same nitrous gas, the absorption was 68 two hundredth parts in place of 55: there results from it always in the same proportion of 0·22 of oxygen contained in atmospheric air, a proportional indication of about 0·30 of this gas in that of the pile. It was tried again with the eudiometer of Volta, by introducing into it a measure through which the electrical spark was made to pass: the trial was repeatedly made upon two, three, and four measures, and always the absorption resulting from the inflammation by the electric spark gave the same indication of about 0·30 of oxygen.

Second Experiment.

Two grammes of distilled water were placed in a glass tube bent in the manner of a syphon. Two wires of the gold of commerce, of about 0·0002 parts of a meter in diameter, were introduced

introduced into this tube, plunging them into the water at about 0·006 parts of a meter distant the one from the other. This tube was placed upon a horizontal pile of fifty double plates of about 0·081 parts of a meter in dimension upon each side. The intervals were filled with dry sand, moistened with river-water acidulated with about a sixtieth part of nitric acid. The gold wires having been placed in communication with the two poles of the pile, the water in the tube assumed, from the first day, a reddish brown colour upon the side of the copper pole, and the wire which joined it was covered with a coating of a deep brown oxide of gold. The wire corresponding with the zinc pole did not assume the same colour. The gold was gradually dissolved, and was precipitated, as well as a portion of the silver alloy. This precipitate presented to the magnifying glass, upon almost the whole length of the tube, needle-formed crystals. The wire corresponding with the zinc pole was entirely cleared of the gold which it contained, and it was now nothing else than a silver wire of extreme tenuity. There was very little gas disengaged from either extremity of the wires. The water was only diminished a fifth part of its volume. The pile was in full activity from the 28th Messidor to the 8th Fructidor: it still indicated to the last day, by means of the electro-micrometer (simplified and perfected by one of the members of the society upon the one constructed in Germany, described in the *Journal de Physique* for the month of Messidor, year 13), a tension of 840 degrees. The liquid residue presented with the different re-agents no mark of acidity, it only had a metallic taste.

The Galvanic Society, by examining principally the results of the first experiment, as relating more particularly to the fact announced by M. Pacchiani, considered that, by keeping account of the small quantity of oxygen which had produced the oxidation of the extremities of the gold wire, they might estimate the total quantity of oxygen contained in the gas of the pile; and, as they found it very nearly in the same proportion that oxygen gas enters into the formation of water, the society believed they might conclude that the only effect of the action of the Galvanic pile, during the whole

whole continuance of the experiment, had been the decomposition of a portion of the water employed, and the separation, in a pure state, of the oxygen and hydrogen gases of which it was formed. The society is therefore of opinion, that M. Pacchiani is deceived respecting the nature of the acid which he announced he had obtained, or that this acid may have come from some animal or vegetable substance employed in his apparatus. They do not hesitate to declare that to the apparatus employed by themselves they give the preference, as the simplest and most remote from any foreign influence; and they do not believe that it is possible to produce any thing by the action of the Galvanic pile, except the decomposition of a greater or less proportion of the water submitted to its action.

XXXI. *Extract of a new Letter of Dr.* Francis Pacchiani, *Professor of Natural Philosophy in the University of Pisa, to M.* Fabroni, *upon the Composition of the Muriatic Acid**.

The efforts which the best naturalists have hitherto made to explain in what manner water is decomposed by means of the electrical column, and to give an account, with precision, of the important questions which have arisen on this subject, demonstrate that the principles from which they set out, in order to attain their object, were far removed from being an immediate result of facts clearly ascertained, and that they were given to the public without regard to the inductions of science.

When hypotheses are established by analogy, if they do not perplex the mind of the philosophical observer, they certainly will be of great assistance to him in his research after truth; but when they are too hastily erected into principles, instead of aiding the judgment, they hinder it from displaying itself, and arriving at those sublime truths which are the object of its labours.

* From *Annales de Chimie*, tome lvi.

I have

I have already sufficiently indicated the method I followed in order to generalize the results I announced; and I have demonstrated that not only gold and platina, but all the metals and metallic bodies, in short all substances proper for decomposing water, as soon as they are traversed by an electrical current strong enough to disengage oxygen, have the property of converting water into oxygenated muriatic acid. This change of nature, this metamorphosis (if I may be permitted so to express myself) of water, fills with astonishment the philosopher who contemplates it, and who comprehends the useful consequences which may be derived from it.

For a long time I have been occupied with this subject, and this result enters into the course of experiments which I made and communicated to M. Viltosio Fossombroni. But have the people who repeated my experiments read my letters with a tranquil spirit, laying aside all the hypotheses already received? Did they make use of the method which I indicated?—Certainly not.

My assertion is so true; that some celebrated chemists, in spite of what I observed in my letters, introduced into the apparatus two metallic wires, making one of them communicate with the positive pole, and the other with the negative pole of the electrical column. How is it possible to obtain, by such a method, the conversion of the water into oxygenated muriatic acid? It is a fact recognised by every naturalist, that the wire which communicates with the positive pole disengages pure oxygen, while the other which communicates with the negative pole disengages from the water very pure hydrogen; it is likewise equally obvious, that the two gases into which the water gradually converts itself, develop themselves in such a proportion, that if they lose their elasticity they will again recompose the same volume of water, equivalent in weight to that of the two gases.

I ask, at present, How, after these facts, it could be pretended that the liquid which remains from the decomposition could convert itself into oxygenated muriatic acid? But let us proceed a step further: the moleculæ of water which are deoxygenated by the contact of the wire of the

positive pole, being deprived of their elasticity, could not they combine themselves immediately with the other moleculæ of water abandoned by hydrogen in the neighbourhood of the wire of the negative pole of the electrical column, since they are equally deprived of elasticity? The thing is self-evident: in fact, if moleculæ of oxygen and hydrogen deprived of elasticity, and in the necessary proportions to form water, could enter into immediate combination and not form water, it would be impossible that water could exist at all.

These considerations did not escape the celebrated Humboldt and Gay-Lussac: they understood extremely well that in the experiment of the English chemists the water could not be oxygenated and hydrogenated but for a single moment alone; seeing that the total absorption of hydrogen on one side, and of oxygen on the other, shows that the water is really neither hydrogenated nor oxygenated; because, in order to become so, it would be necessary that it should absorb one of the two gases in a proportion different from that required for the composition of water: then, if it absorbs these two gases in the proportion indicated, we ought to conceive that the properties of one of these would be neutralized by those of the other, and that consequently, in the experiment quoted, the water might hydrogenate or oxygenate itself for a single moment, but that it could not remain in this state in a permanent manner, for the reason already mentioned.

But to return to our subject, which is to resolve the problem of the solution upon which the conversion of water into oxygenated acid depends.

A volume of water, distilled and deprived of air, being given, decompose it in such a manner that the element of which it ought to clear itself gradually may be very pure oxygen.

Solution.

Take a glass tube of any form you please, provided that it has two orifices, the one small and rounded, the other of a diameter large enough to introduce the water without trouble: through the first of these orifices make a gold wire pass

pass and seal it up with wax; then fill the tube with distilled water, and place in it two or three layers of white linen moistened; seal it up by fixing the linen to the extremity of the tube. Plunge the tube by this last extremity into a vessel containing very pure water. By means of several moistened slips of spongy paper make the water of this vessel communicate with the negative pole of a column sufficiently energetic; and, finally, make the gold or platina wire communicate with the positive pole of the electrical column. The energy of this column being proportioned to the number of pairs of metallic plates, to their state, and to that of the humid conductors, would be, as is well known, proportioned to the capacity of the tube which contains the water under experiment. As soon as the circle is completed, it will establish an uninterrupted circulation, and by this means the water will gradually clear itself of oxygen, passing it off by the wire of gold or platina.

This astonishing change of water into oxygenated muriatic acid creates an agreeable surprise in the mind: *Felix qui potuit rerum cognoscere causas.* After having resolved this important problem, I proposed another to myself, which was as follows:

A volume of water, distilled and freed as much as possible from air, being given, it is proposed to extract the hydrogen from it.

Solution.

Take a glass tube with two orifices, the one straight and without any sharp edge, the other with a stopper, and of a diameter sufficient to introduce the distilled water without trouble. Introduce through the smallest orifice a wire of gold, platina, or other metal, and seal it hermetically with wax. Fill this tube with distilled water freed from air, close the other orifice with fine linen moistened with water folded three or four times; plunge the tube on the side of this second orifice into a vessel containing pure water; plunge into this water slips of spongy paper which communicate by the other extremity to the positive pole of the electrical pile. Finally, make the metallic wire communicate with the negative pole of this same pile. This being done, a

circulation of the electrical fluid, as is well known, will commence, which gradually makes a quantity of air escape near the metallic wire, which, being analysed, is found to be almost wholly pure hydrogen.

By this new method of decomposition we obtain water very much oxygenated, as is positively proved by the experiment I have given in my *Opuscules*.

If that which several philosophical physicians have asserted be true, that oxygen is an excellent remedy in cutaneous diseases, the philanthropist may have recourse to the electrical column to obtain oxygenated water, and make numerous experiments useful to society. In short, what simpler vehicle could we choose, by means of which to introduce oxygen into the human body, than a liquid so necessary to life?

XXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

Feb. 27. The Right Honourable Sir Joseph Banks, President, in the chair.—A paper by Mr. Home, "on a particular affection of the prostate gland," was read. It was illustrated by a drawing exhibiting minutely the situation and figure of this newly discovered gland, or rather peculiar nipple-formed elongation of a part of the substance of the prostate gland protruding against the bladder. This disease, which so painfully affects the bladder, has hitherto been irremediable, chiefly from its true cause not being known; and it is hoped that this physiological discovery may be of incalculable advantage towards relieving the sufferings of patients supposed to be labouring under the effects of calculi and other urinary diseases.

March 6. The President in the chair.—The reading of a communication from Dr. Herschel, "on the quantity and velocity of the solar motion," was commenced.

March 13. The President in the chair.—Continuation of the above paper, much of which was of a nature not to be read, being mathematical tables of the relative distances of the

the fixed stars. The observations contained in this paper are in continuation of a former communication from the author on the direct motion of the sun, and illustrate, with his usual ingenuity, the causes of the sidereal motions applied to stars of six different magnitudes.

March 20. The President in the chair.—A paper by J. Mendoza Rios, esq. was read, explaining the properties and use of an instrument for dividing circles.

SOCIETY OF ANTIQUARIES.

Feb. 27. The right honourable the earl of Leicester, president, in the chair.—The indefatigable Mr. Lysons furnished some very curious extracts from the Tower Records of Edward I., in which it appeared that Edward was extremely attached to different kinds of sports and pastimes, and particularly to a game with cards which he brought from Palestine after his croisade, where painted figures on paper had been in use long before the reputed discovery of cards by a Frenchman in the 14th century. This fact is worthy of attention, as it would lead to the discovery of the real origin of several other things which have been introduced by the same means into Europe, and which have latterly passed, but erroneously, for French discoveries. It will be remembered, that Edward, returning from the croisade, came through France, where he was treacherously detained as a hostage, and during which time he contributed very materially to disseminate the arts and civilize his then faithful subjects of Guyenne. It is also known that the Moors used something nearly similar to cards for amusement at Cordova and Granada, and it is most probable that they were of Carthaginian or Phœnician origin. Like almost all the utensils of civil life, games with cards have doubtless originated in commercial and mercantile countries.

J. P. Malcolm, esq. exhibited to the society the core of a horn that was found in St. Paul's Church-yard, nearly nine feet below the surface of the ground. This bone was supposed to have belonged to some of the animals sacrificed in the days of heathen superstition, as it is alleged that the site of St. Paul's was antiently a place where the Romans offered

up sacrifices to their gods. This exhibition and letter produced such a general emotion, that the venerable locks of the hoary-headed antiquaries shook with laughter, although the very learned author did not pronounce whether the bone which he exhibited had belonged to a *red* or a *white* bull! On the durability of a most porous bone Mr. Malcolm had certainly never reflected, otherwise he would not have supposed it above 1500 years old!

Several historical documents, extracted by Mr. Lysons, were read, relative to the principles and conduct of the last Welsh prince of Wales (in 1260), and his reluctance to submit to the English domination.

March 6. C. Orde, esq. vice-president, in the chair.—Some of the antiquarian friends (not *naturalists*, indeed,) of a learned lady, having expressed their doubts if the misletoe grew on the oak, she ordered a search to be made, and ascertained the fact, which was communicated by letter to the society, and confirmed *viva voce* by one of the members, who saw this parasitic plant attached to an oak branch suspended in the hall of Berkeley castle. Our industrious antiquaries seem to have forgotten that the *viscus quercinus*, or misletoe of the oak, has been used in medicine.

An interesting letter to the president from the reverend S. Weston, B. D. was read, containing a description of a hitherto unknown brass coin of one of the states of antient Greece. On one side was a head, and on the other a bunch of grapes well executed. The following letters only were visible—HTΨ; the coin having been cut into an oval form. The opinions advanced respecting it seemed to rest on mere conjecture.

March 20. The right honourable president in the chair.—A mining instrument, found in an old mine near Castleton, Derbyshire, was exhibited. It is of the figure of a common gouge used by carpenters, about a foot long and an inch in diameter. The upper part of it is covered with spar, which is evidently formed upon it, and adhering to it on all sides. It is thought that this mine has not been wrought since the days of the Saxons; but no opinion was given of the probable antiquity of the instrument, which is unquestionably much

much more modern, and may have been used long since the mine in which it was found has been worked. Nor is the formation of spar one of the slowest processes of mineral aggregation.

GALVANIC SOCIETY, PARIS.

The following account of the proceedings of this society, during the years 1804 and 1805, was read at its sitting on the 6th of February 1806.

The pile of Volta has been considered in different points of view, and has experienced several new modifications in its construction. One of the correspondents of the society, M. Marechaux, has formed a pile of nine disks, composed of zinc and copper, separated by rounds of dry blotting paper. Cords of silk supply the place of the glass tubes in the ordinary apparatus, and hold it suspended to a hook. The pile thus constructed produces very sensible effects. M. Marechaux thought he remarked, by means of this apparatus, that the electrical tension of the pile increases and decreases in proportion to the electrical state of the air, and that its action is stronger the more the air is charged with humidity.

A new apparatus, to which the name of the Galvanic chain has been given, has been invented by M. Struve. This chain is composed of several double cones, one of copper and another of zinc, and so on alternately, soldered together by their bases. To the point of these cones is annexed a hook, which serves to join together a greater or less quantity of these double cones. Linen or cotton is placed between these latter, in such a manner, however, that the extremities are in contact with each of the extremities of zinc and copper. The chain, thus disposed and saturated with muriate of soda, is instantly in a condition to operate. It is, according to the author, less oxidable than the ordinary apparatus, and its activity is triple; a chain of 15 or 20 of these cones having as much energy as a pile of 50 or 60 disks.

It was very difficult, with the straw electrometer of Volta, and even with the electric-balance of Coulomb, to appreciate

the feeble electrical tensions of the Galvanic pile without having recourse to the condenser; an instrument variable in its effects, and of which the results are often deceitful. To remedy this inconvenience, different electrometers have been presented to the society, one by M. Volf, and another by count Sternberg; but none of these seemed to unite so many advantages as one produced by M. Marechaux, as improved by M. Veau-de-Launy.

Messrs. Nauche, Graperon, and Baget, have ascertained that the Galvanic action is augmented, in the first place, when the pile is exposed to a high temperature; secondly, when it is plunged into flame, or into oxygen gas, or carbonic acid gas, &c.; thirdly, they ascertained that the effects of the pile are not transmissible *in vacuo**, or that they are then scarcely perceptible even by means of a condenser.

By analogous experiments M. Edmond de Barrey ascertained the non-transmission of Galvanism through smoke.

Naturalists having announced opposite results from their experiments on the conductibility of flame, M. Izarn took up the question anew. He has ascertained that flame interposed in the Galvanic chain perceptibly transmits the effects of the pile. This he discovered by its action on frogs. This conductible property is nevertheless very feeble, the frogs being excited in a much more lively manner when no flame is interposed.

The diamond, which several modern chemists regard as pure carbon, is, according to the experiments of M. Brugnatelli, a non-conductor of Galvanism; although it is ascertained, from the labours of M. Curtet, of Brussels, that the oxide of carbon is one of the very best conductors.

M. Herman, of Berlin, has examined the properties of different substances employed as Galvanic conductors; and has divided them into insulated bodies, into perfect and im-

* Mr. W. H. Pepys ascertained, a considerable time ago, that the action of the pile is augmented by placing it in oxygen, and destroyed when placed in a vacuum; but, instead of *the effect* not being transmissible through a vacuum, he has ascertained that the metals may be deflagrated *in vacuo*. The metals, of course, are not oxidated—they are only volatilized, light being given out during the process.—Edit.

perfect

perfect conductors, and into uni-polar and bi-polar bodies, according as their conductible property manifests itself at both, or only at one of the extremities of the pile.

The theory of the decomposition of water by means of the apparatus of Volta has commanded the particular attention of the society.

One of the honorary members, M. Chompré, produced this decomposition in a very strong glass tube full of water and hermetically sealed, by means of a brass cap screwed on with leather between the joints. The result of his experiments was, that the Galvanic action takes place as well in the closed tube as in an open one, or one only partly filled with water; but that in the first, the water, in order to make room for the bubbles of the gas produced by decomposition, penetrates through the pores of the brass or greased leather which serve to close the tube, and makes its appearance in spite of every obstacle.

The reported formation of muriatic acid by the decomposition of water presented a new problem for the solution of the society; and numerous experiments have been made in order to ascertain the facts announced by M. Pacchiani. The results, however, have not yet been similar to his; but the society is still occupied in pursuing the same experiments.

M. Marum produced the decomposition of water by means of the grand electrical machine in the Teylerian museum: the two wires of platina employed in the experiment produced the liberation of a mixture of oxygen and hydrogen gases; and he was not able to obtain them separate, as he did with the pile.

Gautherot, whose loss every naturalist deplores, was the first who suggested the idea of accumulating the Galvanic fluid, and preserving it in apparatus where it was not spontaneously produced. He accomplished this by a method at once simple and ingenious.

"If we place," says he, "in a bottle of salt water, closed with a linen stopper, the extremities of two wires of platina, which traverse this same stopper without touching one another; and if we make the outward extremities of these two wires

wires communicate with the two poles of the pile; this communication may be interrupted, and yet the wires will excite a decided taste, and perhaps a slight commotion, and will even produce the decomposition of water. This experiment demonstrates the presence of the Galvanic fluid in an apparatus by no means proper to form it.

" If we plunge the two extremities of a single wire of platina in the extreme cups of the disk apparatus, and if we bring these two ends near together, but without meeting, and carry them to the mouth, we experience a Galvanic taste, the more decided in proportion as the diameter of the wire is more considerable."

The discovery made by this author, and the importance of which he has represented, ought to become, to use his own words, " the source or basis of several other experiments, and coneur, more than any other, to the discovery of the theory of this new branch of physics."

Treading in the footsteps of Gautherot, M. Ritter has proceeded a step further, in ascertaining that bodies which form a part of a Galvanic arc, pass, upon quitting it, into an opposite state from that which they formerly held, in such a manner, that the side, which during the communication was positive, becomes negative when it ceases, and so on, *vice versâ*. This remark conducted him to the construction of a secondary or charging pile,—a happy invention, which forms an epoch in the history of Galvanism.

This pile is formed of disks of a simple metal, such as copper, and of an equal number of cards well saturated with water. It is raised on an ordinary support, by alternately laying on the disks of copper and the rounds of wet card. The whole is kept steady by means of glass rods.

The pile, constructed in this manner, by itself produces no perceptible phænomena; but placed for a few minutes in communication with the Volta's pile, it acquires the properties of that pile, displays an electrical tension, evinces commotion, and gives sensations of light, taste, and other Galvanic phænomena in the same manner.

The society resolved to ascertain the effects of this secondary pile, with all the modifications which its construction

tion presented; and although they obtained results a little weaker than those announced by Ritter, they do not hesitate to regard his labours, as well as those of Gautherot, as the most fortunate experiments which have been made since the invention of the pile of Volta; and they acknowledge that both of these philosophers deserve well of science—the one for having paved the way, and the other for having given a grand discovery to the world.

The effects of Galvanism in medicine have been also tried, but with little success hitherto. In some cases of asphixia it did harm; and out of an immense number of applications to deaf and dumb patients, both naturally and accidentally so, only two seemed to have derived any benefit.

Several poisons having evinced the Galvanic excitability, while others remained unmoved, M. Wranken took that opportunity of ascertaining different kinds of poisons.

UNIVERSITY OF GÖTTINGEN.

This university proposes the following question as the subject of a prize essay, to be given in before July 1807:—"What is the influence of the various taxations, on the morals and industry of the people?"

SOCIETY OF SCIENCES, COPENHAGEN.

Messrs. Chaptal and Cuvier have been elected members of this society.

Professor Treschow, of Copenhagen, has been occupied during the winter in a course of lectures on anthropology, wherein he has severely criticised the speculations of Dr. Gall on the nature of the human soul.

SOCIETY OF SCIENCES, GOERLITZ.

This society has proposed the following subject of investigation to the learned:

1. In cloudy weather it freezes but in a small degree until the thermometer of Reaumur has fallen to the zero point, or at least very little above it: Wherefore then, in a serene sky, does it freeze when the same thermometer is three or four degrees above the zero point?

2. Collect

2. Collect out of the works of Plautus every thing which relates to the knowledge of men and manners of his time, and arrange these materials in such a manner as to produce a picture of civilization and manners at that epoch.

XXXIII. *Intelligence and Miscellaneous Articles.*

VACCINATION.

THE Prussian government has given further encouragement for the progress of vaccination, by ordering medals to be struck of the value of fifty ducats in gold, and containing four ounces of silver each, to be given as prizes to those who contribute to its success.

KEPLER THE ASTRONOMER.

A subscription has been opened at Ratisbon for a monument to the memory of Kepler the astronomer. It is to consist of a Doric temple 23 feet high, and is to be erected in the Sternbergian gardens.

ASTRONOMY.

Table of the right Ascension and Declination of Ceres, Pallas, and Juno, for April 1806.

	Ceres.				Pallas.				Juno.			
	A.R.			Dec.N.	A R.			Dec. S.	A.R.			Dec.N.
1806.	h	m	s	o ′	h	m	s	o ′	h	m	s	o ′
April 3	6	55	4	30 52	5	48	24	5 34	11	2	20	7 12
6	6	58	28	30 47	5	53	44	4 48	11	0	52	7 32
9	7	2	8	30 40	5	59	12	4 4	10	59	40	7 61
12	7	5	52	30 34	6	44	44	3 21	10	58	36	8 8
15	7	9	48	30 26	6	10	20	2 39	10	57	40	8 24
18	7	13	52	30 18	6	16	4	1 59	10	56	56	8 38
21	7	18	0	30 10	6	21	52	1 22	10	56	28	8 50
24	7	22	16	30 1	6	27	48	0 46	10	56	12	9 1
27	7	26	40	29 51	6	33	44	0 12	10	56	4	9 10
30	7	31	12	29 41	6	39	48	0 20N	10	56	12	9 17

Ceres and Pallas are too near the sun to be any longer seen in this country.

MECHANICS.

MECHANICS.

A mechanic of Copenhagen has made a model of a praam intended to conduct, without danger, ships of the largest dimensions across the ice. His model has been examined by the most celebrated engineers on the continent, and promises to be of great service to the Danish marine.

LECTURES.

Mr. Thelwall has opened a seminary for the cultivation of the science and practice of elocution, and the cure of impediments of speech; and has just commenced a course of lectures, at his house, No. 40, Bedford-place, Russel-square, on the physiological principles of his art, and the causes, prevention, and cure of the several species of impediments, natural and habitual. The intention of the lecturer is to treat his subject as a branch of natural and experimental philosophy; to investigate the laws of organic action upon which the phænomenon of speech depend; to consider at large the theory of the human voice, and of human enunciation; and to apply the principles of that theory to the practical improvement of the power and tone of the voice, and the facilities of enunciative expression. For this purpose, besides the more popular accompaniments of reading, recitations, and oratorical digressions, the lectures are illustrated by graphic and mechanical demonstrations of the essential propositions; and an attempt is made to place whatever relates to the fundamental requisites, and even to many of the higher graces of elocutionary expression, on the broad and sure foundations of anatomical and mathematical science. Even our perceptions of musical proportion, and the consequent laws of musical composition, are referred, for their origin, to certain principles of physical necessity, resulting from the structure of the organs of voice: and from the existing harmony between these principles and our perceptions of such proportions, Mr. T. builds his expectations of surmounting all impediments not resulting either from deafness or imbecility of mind. His plan, of course, includes the structure and application of artificial organs, &c., for the relief of those who have absolute deficiencies or malconformations

conformations of the mouth. The lecturer also proposes to receive into his house a limited number of pupils afflicted with impediments, and to give private instructions in all the various branches of elocution.

LIST OF PATENTS FOR NEW INVENTIONS.

A grant unto Patrick Whytock, of Liverpool, in the county palatine of Lancaster, merchant; for his invented improvements in the manufacture of piece goods, composed of cotton, of flax, or of hemp, or of any mixture or mixtures of two or more of these articles, by which such goods will resist the rotting action of wet or moisture much better than similar fabrics manufactured by the methods in common use. Dated March 8.

To John Curr, of Sheffield-Park, in the parish of Sheffield, in the county of York, gentleman; for his invented method, different from any that has hitherto been invented or known, of spinning hemp for the making of ropes or cordage. Dated as above.

To Richard Willcox, of the parish of St. Mary Lambeth, in the county of Surrey, mechanist; for his invented certain machinery for glazing and graining leather, now usually performed by hand. Dated as above.

To Edward Dampier, of Primrose-street, in the city of London, manufacturer; for his invented and brought to perfection certain machinery for rasping, grating, or reducing into small parts or powder, such woods, drugs, and other substances, for the use of dyers and others, as are not easily to be pulverized by mere percussion; and also in regard that he is connected in trade with Edward Jackson and Thomas Shackleton, of Primrose-street aforesaid. Dated March 12.

To Michael Logan, of Paradise-street, in the parish of Rotherhithe, in the county of Surrey, engineer; for his invented and constructed entire new system of marine, fort, and field artillery. Dated March 13.

To Charles Robert West, of Plough-court, Fetter-lane, in the city of London, optician, and William Bruce, of King's-Head-court, Shoe-lane, in the city of London aforesaid, optical turner; for their invented certain improvements in

day

day or night telescopes, whereby the same will be rendered more portable than they now are. Dated March 18.

To Henry Gore Clough, of Norton-street, in the parish of St. Mary-le-bone, and county of Middlesex, surgeon; for his invented certain improvements in the instruments or apparatus commonly called trusses, which are used for compressing and supporting such parts of the human frame as are or may be ruptured or disposed to protrude. Dated March 21.

To Francis Place, of Charing-cross, in the parish of St. Martin in the Fields, in the county of Middlesex, tailor and mercer; for his invented certain improvements in locks for muskets, pistols, fowling-pieces, carriage guns, and every species of fire-arms. Dated as above.

To Richard Ottley, of Myrtle-hill, near Caermarthen, in Caermarthenshire, esq., and James Jeans, of Portsmouth, in the county of Hants, ship-builder; for their invented certain improvements in chain-pumps, in the mode of working the same, and in the wells for receiving such pumps, whereby much manual labour will be saved. Dated as above.

To Joseph Hinchsliffe, of Dumfries, in that part of the united kingdom called Scotland; for his invented new method of manufacturing elastic spring trusses for ruptures or rupture bandages. Dated March 26.

To Bracy Clark, of Giltspur-street, in the city of London, veterinary surgeon; for his invented certain improvements upon horse-shoes. Dated as above.

To Quintin Mc Adam, of Anderston, near the city of Glasgow, in the county of Lanark, in that part of the united kingdom of Great Britain and Ireland called Scotland; for an improved method of dressing yarns for weaving, by means of a new and useful machine. Dated March 26.

To William Parr, of Bermondsey New Road, in the county of Surrey, gentleman, Richard Bevington, of Gracechurch-street, in the city of London, merchant, and Samuel Bevington, of Grange-road, Bermondsey, in the said county of Surrey, leather-dresser; for their invented machine for splitting hides, skins, pelts, or leather, in an improved manner. Dated as above.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For March 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. 11 o'Clock, Night.	Thermometer. Noon.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb. 26	47°	50°	39°	30·12	46°	Fair
27	43	50	36	29·80	42	Fair, and wind
28	34	43	34	30·10	42	Fair, and wind
March 1	35	42	35	·15	15	Cloudy
2	45	50	46	·20	20	Cloudy
3	46	54	46	29·92	15	Showery
4	37	44	34	30·35	10	Fair
5	34	40	32	·36	16	Fair
6	32	37	32	·38	22	Fair
7	35	47	42	·30	26	Cloudy
8	40	44	40	·14	12	Cloudy
9	42	43	32	29·56	0	Rain
10	32	37	32	28·90	0	Snow
11	32	36	28	29·10	6	Cloudy
12	26	30	26	28·98	0	Snow
13	25	36	33	29·35	0	Cloudy
14	37	48	32	·02	0	Rain
15	32	42	33	·51	24	Fair
16	36	36	34	·30	0	Rain
17	36	37	35	·25	0	Rain
18	36	40	39	·35	0	Showery
19	39	46	40	·31	10	Cloudy
20	40	47	41	·42	14	Cloudy
21	40	52	46	·80	30	Fair
22	42	54	44	·85	15	Cloudy
23	46	57	49	·72	24	Cloudy
24	49	54	47	·68	15	Cloudy
25	47	52	47	·60	0	Rain
26	45	49	46	·85	18	Cloudy

N. B. The barometer's height is taken at noon.

XXXIV. *Account of a Series of Experiments, showing the Effects of Compression in modifying the Action of Heat. By Sir* James Hall, *Bart. F.R.S. Edin.*

[Continued from p. 155.]

III. *Experiments made in Tubes of Porcelain.—Tubes of Wedgewood's Ware.—Methods used to confine the Carbonic Acid, and to close the Pores of the Porcelain in a horizontal Apparatus.—Tubes made with a View to these Experiments.—The Vertical Apparatus adopted.—View of Results obtained both in Iron and Porcelain.—The Formation of Limestone and Marble.—Inquiry into the Cause of the partial Calcinations.—Tubes of Porcelain weighed previous to breaking.—Experiments with Porcelain Tubes proved to be limited.*

While I was carrying on the above-mentioned experiments, I was occasionally occupied with another set, in tubes of porcelain. So much, indeed, was I prepossessed in favour of this last mode, that I laid gun-barrels aside, and adhered to it during more than a year. The methods followed with this substance differ widely from those already described, though founded on the same general principles.

I procured from Mr. Wedgewood's manufactory at Etruria, in Staffordshire, a set of tubes for this purpose, formed of the same substance with the white mortars, in common use, made there. These tubes were fourteen inches long, with a bore of half an inch diameter, and thickness of 0·2; being closed at one end (figs. 9, 10, 11, 12, 13*).

I proposed to ram the carbonate of lime into the breech (fig. 9. A); then filling the tube to within a small distance of its muzzle with pounded flint (B), to fill that remainder (C) with common borax of the shops (borate of soda) previously reduced to glass, and then pounded; to apply heat to the muzzle alone, so as to convert that borax into solid glass; then, reversing the operation, to keep the muzzle

* Plate IV., given in our last Number.

cold, and apply the requisite heat to the carbonate lodged in the breech.

I thus expected to confine the carbonic acid; but the attempt was attended with considerable difficulty, and has led to the employment of various devices, which I shall now shortly enumerate, as they occurred in the course of practice. The simple application of the principle was found insufficient, from two causes: 1st, The carbonic acid being driven from the breech of the tube towards the muzzle, among the pores of the pounded silex, escaped from the compressing force, by lodging itself in cavities which were comparatively cold: 2dly, The glass of borax, on cooling, was always found to crack very much, so that its tightness could not be depended on.

To obviate both these inconveniences at once, it occurred to me, in addition to the first arrangement, to place some borax (fig. 10. C) so near the breech of the tube as to undergo heat along with the carbonate (A); but interposing between this borax and the carbonate a stratum of silex (B), in order to prevent contamination. I trusted that the borax in a liquid or viscid state, being thrust outwards by the expansion of the carbonic acid, would press against the silex beyond it (D), and totally prevent the elastic substances from escaping out of the tube, or even from wandering into its cold parts.

In some respects this plan answered to expectation. The glass of borax, which can never be obtained when cold, without innumerable cracks, unites into one continued viscid mass in the lowest red heat; and as the stress in these experiments begins only with redness, the borax, being heated at the same time with the carbonate, becomes united and impervious as soon as its action is necessary. Many good results were accordingly obtained in this way. But I found, in practice, that as the heat rose, the borax began to enter into too thin fusion, and was often lost among the pores of the silex, the space in which it had lain being found empty on breaking the tube. It was therefore found neces-

sary to oppose something more substantial and compact, to the thin and penetrating quality of pure borax.

In searching for some such substance, a curious property of bottle-glass occurred accidentally. Some of this glass, in powder, having been introduced into a muffle at the temperature of about 20° of Wedgewood; the powder, in the space of about a minute, entered into a state of viscid agglutination, like that of honey, and in about a minute more (the heat always continuing unchanged) consolidated into a firm and compact mass of Reaumur's porcelain *. It now appeared, that by placing this substance immediately behind the borax, the penetrating quality of this last might be effectually restrained; for Reaumur's porcelain has the double advantage of being refractory, and of not cracking by change of temperature. I found, however, that in the act of consolidation, the pounded bottle-glass shrunk, so as to leave an opening between its mass and the tube, through which the borax, and along with it the carbonic acid, was found to escape. But the object in view was obtained by means of a mixture of pounded bottle-glass and pounded flint, in equal parts. This compound still agglutinates, not indeed into a mass so hard as Reaumur's porcelain, but sufficiently so for the purpose; and this being done without any sensible contraction, an effectual barrier was opposed to the borax; (this arrangement is shown in fig. 11.;) and thus the method of closing the tubes was rendered so complete, as seldom to fail in practice †. A still further refinement upon this method was found to be of advantage. A second series of powders, like that already described, was introduced towards the muzzle, as shown in fig. 12. During the first period of the experiment, this last-mentioned series was exposed to heat, with all the outward half of the tube (*a b*);

* In the same temperature a mass of the glass of equal bulk would undergo the same change; but it would occupy an hour.

† A substance equally efficacious in restraining the penetrating quality of borax, was discovered by another accident. It consists of a mixture of borax and common sand, by which a substance is formed, which, in heat, assumes the state of a very tough paste, and becomes hard and compact on cooling.

by this means a solid mass was produced, which remained cold and firm during the subsequent action of heat upon the carbonate.

I soon found that, notwithstanding all the above-mentioned precautions, the carbonic acid made its escape, and that it pervaded the substance of the Wedgewood tubes, where no flaw could be traced. It occurred to me, that this defect might be remedied, were borax, in its thin and penetrating state of fusion, applied to the inside of the tube; and that the pores of the porcelain might thus be closed, as those of leather are closed by oil in an air-pump. In this view I rammed the carbonate into a small tube, and surrounded it with pounded glass of borax, which, as soon as the heat was applied, spread on the inside of the large tube, and effectually closed its pores. In this manner, many good experiments were made with barrels lying horizontally in common muffles (the arrangement just described being represented in fig. 13).

I was thus enabled to carry on experiments with this porcelain, to the utmost that its strength would bear. But I was not satisfied with the force so exerted; and, hoping to obtain tubes of a superior quality, I spent much time in experiments with various porcelain compositions. In this I so far succeeded as to produce tubes, by which the carbonic acid was, in a great measure, retained without any internal glaze. The best material I found for this purpose was the pure porcelain clay of Cornwall, or a composition in the proportion of two of this clay to one of what the potters call Cornish stone, which I believe to be a granite in a state of decomposition. These tubes were seven or eight inches long, with a bore tapering from 1 inch to 0·6 Their thickness was about 0·3 at the breech, and tapered towards the muzzle to the thinness of a wafer.

I now adopted a new mode of operation, placing the tube vertically, and not horizontally as before. By observing the thin state of borax whilst in fusion, I was convinced that it ought to be treated as a complete liquid, which being supported in the course of the experiment from below, would

secure

secure perfect tightness, and obviate the failure which often happened in the horizontal position, from the falling of the borax to the lower side.

In this view (Plate VII. fig. 16.), I filled the breech in the manner described above, and introduced into the muzzle some borax (C) supported at the middle of the tube by a quantity of silex mixed with bottle-glass (B). I placed the tube so prepared, with its breech plunged into a crucible filled with sand (E), and its muzzle pointing upwards. It was now my object to apply heat to the the muzzle half, whilst the other remained cold. In that view, I constructed a furnace (fig. 14. and 15.), having a muffle placed vertically (*cd*), surrounded on all sides with fire (*ee*), and open both above (at *c*), and below (at *d*). The crucible just mentioned, with its tube, being then placed on a support directly below the vertical muffle (as represented in fig. 14. at F), it was raised so that the half of the tube next the muzzle was introduced into the fire. In consequence of this, the borax was seen from above to melt, and run down in the tube, the air contained in the powder escaping in the form of bubbles, till at last the borax stood with a clear and steady surface like that of water. Some of this salt, being thrown in from above, by means of a tube of glass, the liquid surface was raised nearly to the muzzle, and, after all had been allowed to become cold, the position of the tube was reversed; the muzzle being now plunged into the sand (as in fig. 17.), and the breech introduced into the muffle. In several experiments I found it answer well, to occupy great part of the space next the muzzle with a rod of sand and clay previously baked (fig. 19. KK), which was either introduced at first, along with the pounded borax, or, being made red hot, was plunged into it when in a liquid state. In many cases I assisted the compactness of the tube by means of an internal glaze of borax; the carbonate being placed in a small tube (as shown in fig. 18).

These devices answered the end proposed. Three-fourths of the tube next the muzzle was found completely filled with a mass, having a concave termination at both ends, (*f* and *g*, figs. 17, 18, 19.) showing that it had stood as a liquid

in the two opposite positions in which heat had been applied to it. So great a degree of tightness, indeed, was obtained in this way, that I found myself subjected to an unforeseen source of failure. A number of the tubes failed, not by explosion, but by the formation of a minute longitudinal fissure at the breech, through which the borax and carbonic acid escaped. I saw that this arose from the expansion of the borax when in a liquid state, as happened with the fusible metal in the experiments with iron barrels; for the crevice here formed indicated the exertion of some force acting very powerfully, and to a very small distance. Accordingly, this source of failure was remedied by the introduction of a very small air tube. This, however, was used only in a few experiments.

In the course of the years 1801, 1802, and 1803, I made a number of experiments, by the various methods above described, amounting, together with those made in gun-barrels, to one hundred and fifty-six. In an operation so new, and in which the apparatus was strained to the utmost of its power, constant success could not be expected; and, in fact, many experiments failed, wholly or partially. The results, however, upon the whole, were satisfactory, since they seemed to establish some of the essential points of this inquiry.

These experiments prove, that, by mechanical constraint, the carbonate of lime can be made to undergo strong heat without calcination, and to retain almost the whole of its carbonic acid, which, in an open fire, at the same temperature, would have been entirely driven off; and that, in these circumstances, heat produces some of the identical effects ascribed to it in the Huttonian theory.

By this joint action of heat and pressure, the carbonate of lime, which had been introduced in the state of the finest powder, is agglutinated into a firm mass, possessing a degree of hardness, compactness, and specific gravity*, nearly approaching to these qualities in a sound limestone; and some of the results, by their saline fracture, by their semi-trans-

* See Appendix—(to be given in a future Number.)

parency,

pareney, and their susceptibility of polish, deserve the name of marble.

The same trials have been made with all calcareous substances; with chalk, common limestone, marble, spar, and the shells of fish. All have shown the same general property, with some varieties as to temperature. Thus I found, that, in the same circumstances, chalk was more susceptible of agglutination than spar; the latter requiring a heat two degrees higher than the former, to bring it to the same pitch of agglutination.

The chalk used in my first experiments always assumed the character of a yellow marble, owing probably to some slight contamination of iron. When a solid piece of chalk, whose bulk had been previously measured in the gage of Wedgewood's pyrometer, was submitted to heat under compression, its contraction was remarkable, proving the approach of the particles during their consolidation; on these occasions, it was found to shrink three times more than the pyrometer-pieces in the same temperature. It lost, too, almost entirely, its power of imbibing water, and acquired a great additional specific gravity. On several occasions I observed, that masses of chalk, which, before the experiment, had shown one uniform character of whiteness, assumed a stratified appearance, indicated by a series of parallel layers of a brown colour. This circumstance may hereafter throw light on the geological history of this extraordinary substance.

I have said, that, by mechanical constraint, almost the whole of the carbonic acid was retained. And, in truth, at this period some loss of weight had been experienced in all the experiments, both with iron and porcelain. But even this circumstance is valuable, by exhibiting the influence of the carbonic acid, as varied by its quantity.

When the loss exceeded 10 or 15 per cent.* of the weight of the carbonate, the result was always of a friable texture,

* I have found that, in open fire, the entire loss sustained by the carbonate varies in different kinds from 42 to 45·5 per cent.

and without any stony character; when less than 2 or 3 per cent., it was considered as good, and possessed the properties of a natural carbonate. In the intermediate cases, when the loss amounted, for instance, to 6 or 8 per cent., the result was sometimes excellent at first, the substance bearing every appearance of soundness, and often possessing a high character of crystallization; but it was unable to resist the action of the air; and, by attracting carbonic acid or moisture, or both, crumbled to dust more or less rapidly, according to circumstances. This seems to prove, that the carbonate of lime, though not fully saturated with carbonic acid, may possess the properties of limestone; and perhaps a difference of this kind may exist among natural carbonates, and give rise to their different degrees of durability.

I have observed, in many cases, that the calcination has reached only to a certain depth into the mass; the internal part remaining in a state of complete carbonate, and, in general, of a very fine quality. The partial calcination seems thus to take place in two different modes. By one, a small proportion of carbonic acid is taken from each particle of carbonate; by the other, a portion of the carbonate is quite calcined, while the rest is left entire. Perhaps one result is the effect of a feeble calcining cause, acting during a long time, and the other of a strong cause, acting for a short time.

Some of the results which seemed the most perfect when first produced, have been subject to decay, owing to partial calcination. It happened, in some degree, to the beautiful specimen produced on the 3d of March 1801, though a fresh fracture has restored it.

A specimen, too, of marble, formed from pounded spar, on 15th May 1801, was so complete as to deceive the workman employed to polish it, who declared, that, were the substance a little whiter, the quarry from which it was taken would be of great value, if it lay within reach of a market. Yet, in a few weeks after its formation, it fell to dust.

Numberless specimens, however, have been obtained which resist the air, and retain their polish as well as any marble.

Some

Some of them continue in a perfect state, though they have been kept, without any précaution, during four or five years. That set, in particular, remain perfectly entire, which were shown last year in this society, though some of them were made in 1799, some in 1801 and 1802, and though the first eleven were long soaked in water, in the trials made of their specific gravity.

A curious circumstance occurred in one of these experiments, which may hereafter lead to important consequences. Some rust of iron had accidentally found its way into the tube: ten grains of carbonate were used, and a heat of 28° was applied. The tube had no flaw; but there was a certainty that the carbonic acid had escaped through its pores. When broken, the place of the carbonate was found occupied, partly by a black slaggy matter, and partly by sphericles of various sizes, from that of a small pea downwards, of a white substance, which proved to be quicklime; the sphericles being interspersed through the slag, as spar and agates appear in whinstone. The slag had certainly been produced by a mixture of the iron with the substance of the tube; and the spherical form of the quicklime seems to show that the carbonate had been in fusion along with the slag, and that they had separated on the escape of the carbonic acid.

The subject was carried thus far in 1803, when I should probably have published my experiments, had I not been induced to prosecute the inquiry by certain indications, and accidental results, of a nature too irregular and uncertain to meet the public eye, but which convinced me that it was possible to establish, by experiment, the truth of all that was hypothetically assumed in the Huttonian theory.

The principal object was now to accomplish the entire fusion of the carbonate, and to obtain spar as the result of that fusion, in imitation of what we conceive to have taken place in nature.

It was likewise important to acquire the power of retaining all the carbonic acid of the carbonate, both on account of the fact itself, and on account of its consequences; the result

result being visibly improved by every approach towards complete saturation. I therefore became anxious to investigate the cause of the partial calcinations which had always taken place, to a greater or a less degree, in all these experiments. The question naturally suggests itself,—What has become of the carbonic acid separated in these partial calcinations from the earthy basis? Has it penetrated the vessel, and escaped entirely; or has it been retained within it in a gaseous, but highly compressed state? It occurred to me, that this question might be easily resolved, by weighing the vessel before and after the action of heat upon the carbonate.

With iron, a constant and inappreciable source of irregularity existed in the oxidation of the barrel. But with porcelain the thing was easy; and I put it in practice in all my experiments with this material, which were made after the question had occurred to me. The tube was weighed as soon as its muzzle was closed, and again after the breech had been exposed to the fire; taking care, in both cases, to allow all to cool. In every case I found some loss of weight, proving that, even in the best experiments, the tubes were penetrated to a certain degree. I next wished to try if any of the carbonic acid separated, remained within the tube in a gaseous form; and in that view, I wrapped the tube, which had just been weighed, in a sheet of paper, and placed it, so surrounded, on the scale of the balance. As soon as its weight was ascertained, I broke the tube by a smart blow, and then replaced upon the scale the paper containing all the fragments. In those experiments, in which entire calcination had taken place, the weight was found not to be changed, for all the carbonic acid had already escaped during the action of heat. But in the good results I always found that a loss of weight was the consequence of breaking the tube.

These facts prove, that both causes of calcination had operated in the porcelain tubes; that, in the cases of small loss, part of the carbonic acid had escaped through the vessel, and that part had been retained within it. I had in

view

view methods by which the last could be counteracted; but I saw no remedy for the first. I began, therefore, to despair of ultimate success with tubes of porcelain *.

Another circumstance confirmed me in this opinion. I found it impracticable to apply a heat above 27° to these tubes, when charged as above with carbonate, without destroying them, either by explosion, by the formation of a minute rent, or by the actual swelling of the tube. Sometimes this swelling took place to the amount of doubling the internal diameter, and yet the porcelain held tight, the carbonate sustaining but a very small loss. This ductility of the porcelain in a low heat is a curious fact, and shows what a range of temperature is embraced by the gradual transition of some substances from a solid to a liquid state; for the same porcelain which is thus susceptible of being stretched out, without breaking, in a heat of 27°, stands the heat of 152°, without injury, when exposed to no violence, the angles of its fracture remaining sharp and entire.

[To be continued.]

XXXV. *A new Fact in Galvanism. Communicated by a Correspondent.*

To Mr. Tilloch.

SIR,

WISHING to promote Galvanic inquiries (when unattended with cruelty), I shall beg the favour of you to insert in your Magazine the following circumstances, which I do not know have been observed before:

In trying to produce the nervous flashings by resting different parts of the face on a cylinder of wood covered with tin-foil, I was surprised by perceiving, when the nose was on this cylinder, that there was a strong disagreeable smell, like that which is produced in making inflammable air (hy-

* I am nevertheless of opinion, that, in some situations, experiments with compression may be carried on with great ease and advantage in such tubes. I allude to the situation of the geologists of France and Germany, who may easily procure, from their own manufactories, tubes of a quality far superior to any thing made for sale in this country.

drogen

drogen gas) in the common way, with sulphuric acid, water, and iron filings. This result I experienced several times; the cylinder resting on the copper end of a Galvanic trough consisting of 25 pairs of plates, 2¼ inch. When the nose was placed on a cylinder connected with the zinc end, no such smell was produced. *Quere*, How was the inflammable air (if any was) produced to make so strong a smell? The first time I perceived this smell was on Saturday last. I again tried the experiment this evening, and found the same result. I likewise tried it with two troughs, consisting in all of 50 pairs of plates; and then also perceived the smell: but the experiment was from the number of plates so very unpleasant (the shock being great) that I did not continue it very long, and did not determine whether the smell was proportionally strong.

With these 50 pairs of plates another effect occurred, which I had never seen before. I introduced in the circuit a small glass tube filled with milk instead of water, and whilst it remained in its place I did not see any alteration take place; but when I took out the wire, which was a silver one, and had been connected with the zinc end, I found a sort of crust adhering to it which looked like cream. *Quere*, Was not this curd formed by acid from the zinc end? A gold wire, which was connected with the copper end, I neglected to examine.

April 7, 1806. *A Friend to Physical Inquiries.*

XXXVI. *On Vaccination. An Examination of several of the Mis-statements of Dr.* Rowley. *By Mr.* J. J. Hawkins, *of Islington.*

To Mr. Tilloch.

SIR,

Having for some years seen the most beneficial effects from the introduction of the cow-pock, as well from the practice of many of my friends, as with several I have myself vaccinated, I felt it a duty, on the first publication of Dr. Rowley's pamphlet, entitled "Cow-Pox Inoculation no Security

Security against Small-Pox Infection," to examine some of his alleged cases, in order that I might judge for myself, whether I ought to continue favourable, or become adverse to the new inoculation.

It was at first my intention to examine the whole of his cases; but, hearing that the indefatigable Dr. Thornton had undertaken the task, and knowing he was better qualified than myself, I relinquished it, after visiting ten of the 218 cases detailed in Dr. Rowley's publication. Of these ten, there were three grossly misrepresented; four could not be proved to have gone regularly through the cow-pock; of one no information could be had, the father of the child having died a twelvemonth before Dr. Rowley published the case; and two were most pointedly in favour of vaccination.

Dr. Rowley's scandalous pamphlet would have been totally unworthy of the least notice had he not addressed himself to the passions, and thereby wrought upon the credulity of the ignorant and unthinking; which renders it necessary that the cause of truth and humanity should be vindicated.

I understand there are several replies to Dr. Rowley, two only of which have come into my hands; one, a most excellent satirical piece, by "*Aculeus*;" the other by Dr. Thornton, two reports only of whose labours are as yet published: so far as he has already gone, he has entirely disproved and overturned Dr. Rowley's statements, and exposed the baseness of antivaccinarian proceedings.

From my own observations and practice, I can truly say, I have met with nothing but the most satisfactory evidence in favour of vaccine inoculation, and therefore I conceive it my duty to promote it by all the means in my power. Although the subject is so well handled by the two authors just mentioned, I think it will be of use to contribute my mite to the general stock of evidence, and shall be glad you will insert in your valuable Magazine the following statement of my examination of the ten cases:

Case 1. The case of Marianne Lewis, which is the 88th in Dr. Rowley's list, and the subject of one of his engravings, I examined with very particular attention.

The doctor states, that she was vaccinated "at the Small-

Pox

Pox Hospital *April* 1803, and that in June 1804 she broke out in the head, ears, and chest, with cow-pox mange, cow-pox blueish abscesses: *when the cold weather came, they were better.* In May 1805, the same cow-pox mange, cow-pox abscesses, blueish in appearance, attacked the child in every part of the body, from the head to the foot; there were nothing but cow-pox mange, cow-pox gatherings of matter, cow-pox ulcers, excoriations: the child was a mere Lazarus, covered all over with sores and disease; a most disgusting spectacle of terrible disease."

On my visiting this child in October 1805, the mother told me that it was vaccinated in *May* 1803. There were about 200 vaccinated at the same time. She never carried her to be seen afterwards. *There was no regular pustule, but only a little sore without inflammation, which soon healed up.* The child was particularly healthy till April 1804, when she had eruptions on the head and breast, which continued *four months.* She was then *quite well,* till May last, when the eruptions came again, and extended all over the body: they are now, for the most part, healed. She says also, that Dr. Rowley gave it as his opinion, that the sore was neither cow-pock nor small-pox, but something between both.

From both these statements it appears that the child was inoculated for the cow-pock, and afterwards had an eruption; but the doctor has carefully omitted that, which would have made it no evidence in his favour, namely, that the child *did not take the cow-pock,* although inoculated for it, and that, for eleven months after the inoculation, she was *particularly healthy.*

There is some difference also in the time of the eruption: the doctor states it to commence in June, and to become *better* in *cold weather*; but the mother says it commenced in April, and continued four months, (*till August, the hottest month in the year,*) when the child was *quite well,* till the May following.

From my own examination I am induced to believe that the eruption was very far from being all over the body; but it was sufficient for the doctor's purpose, if he could any way twist facts to favour the antivaccine principles, for

for which end he has most grossly misrepresented this case.

Case 2. Mr. Joules's son, Dr. Rowley's 36th case, stated as follows:—"Vaccinated at the Small-Pox Hospital; terrible tumour in the face, of which a drawing is given resembling an ox."

I saw this child, with his parents, in November 1805. The father told me that the boy was vaccinated four or five years since: soon afterwards he had a slight breaking out on the face, which continued about a month: from that time he was perfectly well for three or four years, when a swelling came in his cheek, but was attended with so little pain, that it did not hinder his playing in the streets as much as other boys. Mr. Joules said, Dr. Rowley promised to undertake the cure gratis, but never troubled himself about it after he had the boy's picture. *The father and mother both declared they did not think the swelling was caused by the cow-pock.* It appeared to me a scrophulous case, and nothing could be further fetched than the notion of its resembling an ox.

Case 3. Mr. Wild's child, the doctor's 133th case, who, he says, took the small-pox, in the natural way, in August 1805, although vaccinated *two years before.*

I saw the mother and child in October 1805. She said the child was vaccinated *three or four years before* it took the small-pox; but the place of inoculation was very small, and did not appear like the cow-pock she had seen in other persons; from whence it is evident, although the child was inoculated for the cow-pock, it did not receive it. Is it any wonder then, that the small-pox infection took place when the child was exposed to it? Certainly not. And it is therefore a gross misrepresentation of the case, to call it an instance of cow-pock failure.

Case 4. Mr. Colson's grandson, Dr. Rowley's 137th case. This he states took the small-pox two years after cow-pox.

In October 1805, I saw Mr. Colson, the grandfather, and also the child's mother, who was but seventeen years of age when her child was vaccinated; and thoughtlessly neglected

to

to carry it to the Skinner-street station, where it was inoculated, to be examined so often as required by the rules of the institution. This, she told me, was entered in the books at the station. It therefore cannot be known that the child went regularly through the cow-pock.

Cases 5 and 6. Mrs. Little's two children, Dr. Rowley's 77th case. He states the small-pox happening three years after vaccination: but from every information I could obtain from the mother and others, there is no proof that they passed regularly through the cow-pock, and consequently they ought not to have been brought forward as cases against vaccination.

Case 7. Mr. Nicholson's boy, the doctor's 35th case: "Small-pox two years after vaccination." The regularity of the vaccination is not proved; and there is no other evidence of the child's having the small-pox, than Dr. Rowley's saying there was one small-pock on its posteriors, from which he *could* have taken matter!

Case 8. Mr. Rice's child, case 197 of Dr. Rowley. I could obtain no other information, than that Mr. Rice had been dead above a year, and the family removed, but could not learn where.

Cases 9 and 10. Elizabeth and William Keen, Dr. Rowley's 37th and 38th cases; both of which he states thus: "Vaccinated May 10th, 1805; small-pox 29th of May."

I saw these two children, with their mother, in November 1805. Her relation was as follows:—She has three children, one of whom took the small-pox in the natural way, and had them very severely; and there was no doubt of the other two having caught the infection; but, by the advice of some friends, she had them vaccinated, hoping thereby to lessen the virulence of the small-pox. The result justified the proceedure: the two vaccinated children passed through the small-pox so favourably, that their healths were scarcely impaired, and it was not easy to determine which of the two diseases predominated in the constitution; whereas the one that was not vaccinated, languished under a most distressing confluent small-pox, which left numerous indelible marks all over the face; besides a large and disgusting scar

scar on the neck, just above the right clavicula; together with a general debility, which six months of time had hardly lessened.

Thus is seen, that these two cases, which Dr. Rowley has, by misrepresentation, endeavoured to warp to his own purposes, are most pointedly and strongly favourable to vaccination: how far it is so with the rest on his black list, will no doubt appear soon, from the labours of Dr. Thornton. I have much reason to believe they will, for the most part, be proved to be misrepresentations of facts, in themselves neither for nor against vaccination: some few mistakes may have arisen from the inexperience of the early vaccinators,—this was to be expected by every rational person: many of the cases will be found highly favourable to vaccination; but a considerable number of them contain their own refutation, which any one who reads with attention may discover. I am, sir,

Your humble servant,

Islington,
March 24, 1806.

JOHN J. HAWKINS.

XXXVII. *On the Method of extracting Spirits from Potatoes. By M.* GERMAIN, *Chemist to the Military Hospital at Hanau**.

IT has been the practice, for a long time past, in Germany to distil spirits from potatoes. In the eastern part of Prussia, and in Lithuania, they employ an immense quantity of these vegetables in distillation. In these countries they are generally planted as the first crop in grounds which had been formerly untilled; and with proper care, and in good seasons, they produce abundantly. The residue, after distillation, is an excellent drink for cattle, particularly cows, whose milk is greatly increased by the use of it. When potatoe spirit is properly distilled, if not mixed with any foreign matter, and if the potatoes have not been heated too much, or burnt, during their preparation, it has a taste and

* From *Annales de Chimie*, tome lvi. p. 207.

flavour far superior to the spirit produced from barley or oats, which is preferred only from custom.

It has been said that potatoe spirit sours easily, and is spoiled upon crossing the line: as I have not had an opportunity of proving the contrary, I have nothing to say at present to these two objections; I know, however, for certain, that it has been preserved in good condition for eighteen months, and that, according to the areometer of Richter*, regulated for the experiment, it marked the 35th degree, without having lost any of its good qualities, or being soured. From the result of this experiment I have every reason to believe that this spirit, if well prepared, is no more subject to the two inconveniences with which it is reproached, than that produced from grain, and that every thing which has been said against it has arisen from prejudice.

Method of performing the Operation of producing Potatoe Spirit.

A sufficient quantity of malt must be added to the potatoes; for instance, 100 bushels of potatoes require 17 bushels and a half of malt; and this quantity will produce five hogsheads of spirits, which, according to the areometer of Richter, marks from the 36th to the 38th degree.

I know also, by experience, that out of 120 bushels of potatoes, and 10 of malt, we obtain the same quantity of spirit, and of the same strength. It may be thought, perhaps, that to produce the same quantity of spirits from potatoes, it requires a larger proportion of fermenting materials than common grain does, as well as more room, a greater number of casks, and a greater expense of firing: but all this is a mistake; because the same casks which contain a determinate quantity of grain will contain an equal quantity of potatoes and malt. These will also produce the same

* Richter's areometer is the same as that of Baumé, with this exception, that each degree of the former instrument is constructed after experiments expressly made for the purpose. The scale is graduated into 100 parts, in such a manner that the number at the level of the liquid denotes the quantity of alcohol. Thus, 36, indicates 36 parts of alcohol in 100 parts of the liquid.

quantity of spirits, provided care is taken not to dilute the potatoes so much as grain, because they have not the same property of swelling which grain has; and provided also, that a good fermentation has been produced, and the spirits have not been burned in the still. As for the fewel required to prepare the potatoes, the additional expense is trifling, although, in every case, boiling water is made use of. Water in this state is used to prepare the potatoes, as the operation is performed by means of the steam of the water, which it is necessary to keep boiling half an hour or three quarters longer, according to the quantity of potatoes employed; and this is the only additional expense which may be reckoned upon.

The preparation of the potatoes must be carried on in vessels made of oak, the staves of which ought to be very thick and solid, and the bottom bound round with iron, in order to guard against accidents in removing. The top of the vessel must have a square aperture, with a thick covering, which should fit exactly; this aperture serves to let the potatoes into the vessel after being well washed: there ought to be another smaller aperture in the side, with a covering to shut close, for the purpose of drawing the potatoes out of the vessel.

It is then placed upon a tressel by the side of a still, different from that which is used for general purposes. On the same side, *i. e.* opposite to the still, and a little above the lower part of the tun, there is an aperture into which the beak of the still is inverted, by means of which the steam is conveyed to the potatoes. In the centre of the bottom of the tun there should be another small aperture, through which to evacuate any thick fluids which may collect in the tun; and in order that the weight of the potatoes may not choke it, the cover should be made to open inwards. When the potatoes are prepared, which the workmen will easily discover by means of the apertures in the tun already described, the beak of the alembic is withdrawn: the potatoes are immediately afterwards ground by a machine, or a kind of handmill, placed before the tun close to the small side-aperture. This mill is composed of two cylinders of very

hard wood, or stone, which may be drawn more or less together, as occasion requires, by means of a wheel and a handle to it, which serves to drive the axles of the cylinders together.

Above the cylinders there is a trough or hopper, into which the potatoes are put after being drawn out of the tun, by little and little, by means of a shovel; and being bruised by the action of the cylinders in this trough, they fall immediately into a tub placed below them. What renders this tub indispensable, is, that below each cylinder there is an iron scraper, to detach the boiled potatoes which may adhere to the cylinders.

When the potatoes are thus prepared, the grated barley is put into a tub and diluted with lukewarm water, taking care not to dilute it too much: the potatoes are then mixed with it by tubfulls as they are ground, and when they are finished the necessary quantity of water is added, and both ingredients are stirred until perfectly well mixed, and not the least lump left. The liquor is then left to settle; stirring it, however, at intervals until the whole is cold, and in a proper state to receive the yeast.

In some places beer yeast is used; but in others an artificial ferment is prepared, composed simply of clean ground rice. This last yeast is prepared by kneading the ground rice in cold water; boiling water is then added until a thick broth is formed. All the efficacy of this preparation results from the care taken in heating it: if too much or too little heated, the whole mass will be spoiled.

To conclude:—It may be observed that potatoes ferment much more easily than grain, and require less yeast; the fermentation besides is very strong, and produces a great quantity of froth; but it does not operate alike through the whole of it, because, in particular places, the gross and membranous part of the potatoes forms a strong crust above, through which the froth cannot penetrate so easily. Experience shows that, upon distilling potatoe spirits with carrots and beet-root, the spirit then drawn is better and more abundant than when made with potatoes and beet-root alone; and the advantages reported to have been derived from

from this last method of proceeding have not been confirmed: on the contrary, it is completely proved, that the addition of carrots gives the spirits an exquisite taste and flavour.

Chemists pretend that it is the saccharine substance which causes the vinous fermentation; that the more of this substance any body contains, the better adapted it is for fermentation. The present experiments on potatoes seem to prove that this assertion is not strictly correct; for they contain no saccharine substance, but merely starch, and yet they ferment. We see in corn, that the quantity of spirits is in proportion to that of the starch, or perhaps the glutinous substance which it contains: wheat, for instance, which contains both the one and the other, in greatest abundance, yields also the greatest quantity of spirits.

The opinion of those who assert that corn in germinating acquires thereby a mildness, seems to me of no weight, because hitherto little light has been thrown on the subject; which has not been yet exhausted by rigorous experiments comparing the different kinds of grain, germinated and not germinated. There are some very intelligent distillers, who still doubt if a determinate quantity of grain produces a greater quantity of spirits because that grain has germinated.

XXXVIII. *Means of destroying the Insects and Caterpillars which attack Fruit Trees.* By Madan Gaçon Dufour*.

We know, by sad experience, that the husbandman has every year some accidents which unexpectedly diminish or destroy the produce of his ground, without having to reproach himself with any want of care or attention.

The present year, for instance (1805), offers a singularity which I have not before perceived. In some districts the cherry-tree has experienced, at the time of its blossoming,

* From *Bibliothèque Physique Economique*, No. 12, August 1805.

colds and winds which have prevented it from setting; but another plague, not less disastrous, has attacked the cherry-trees and plum-trees over several districts in France. Great swarms of little animals resembling *vine-fretters*, but which are not so in reality, established their habitations at the extremity of the branches of the cherry-trees. As soon as a branch was attacked, the leaves curled, and the juice was dried up. On opening the leaf a considerable number of ants was discovered, which, jointly with the insect which began the ravages, sucked the branch, and made it wither. What I have remarked is, that usually, when the vine-fretters attack any tree, the neighbouring tree very soon experiences the same fate; but the attack of this year is only partial. In an alley of cherry-trees which I possess there have seven been attacked, but not those which are next each other. One tree was placed between two which were very much damaged by these insects, and yet this one was not hurt.

On these vermin the smoke of tobacco had no effect at all: this convinces me that they are different from the ordinary kind.

Plum-trees, when attacked by the same insect, do not lose their fruit like the cherry-trees; but the little animals cover them with more rapidity, so as to extirpate even the appearance of fruit.

Having effectually watered a low plum-tree, I covered it with ashes, in the manner we treat beans and cabbages, and the vermin were destroyed: but this is only practicable with a tree of low height.

I made one remark, which I think is essential to communicate: it is, that plum-trees planted in ground which is not necessarily watered, are less attacked by these insects than those which have experienced a humidity communicated by the plants in their neighbourhood, to which watering is absolutely necessary. I had one planted in a bed of artichokes: we know very well that this plant requires plenty of water; and the tree was entirely covered with insects. Its leaves withered, and the fruit fell off; while two other plum-

trees,

trees, in ground not watered at all, were much less attacked. This convinces me that these were not the ordinary vermin abundant in dry seasons.

I was only able to protect my cherries a little, by cutting off the extremities of the damaged branches.

Several people had recourse to sulphur; but I did not follow that method. The smoke of sulphur destroys the insect, I admit, but it is at least equally dangerous to the tree; I always prefer an aspersion of the tree with soap-suds. This very year I experienced the good effects of it. I saw my plum-trees look green again, and the insects abandon them. The aspersion is very easily managed, by means of watering-pots or small garden-engines. I have also employed a ley of wood-ashes with the same success as soap and water.

An observation equally important which I have made is, the great damage done this season in all orchards by the caterpillar. As soon as they devoured the young leaves they attacked the fruit. In spite of the great care taken in spring to get rid of them, the number of these insects is incredible. I have seen them unite on the large branches, fix their nests to them, and protect them by means of the downy matter which covers the buds of the ensuing season. Whatever precaution is taken, it is almost impossible not to destroy these buds. It is only necessary to take off these nests and burn them; and this is the only way of getting rid of the coveys. I employed the same aspersion for my apple-trees, and by that means got rid of their enemies also.

XXXIX. *Experiments upon the Gaseous Oxide of Azote, made at a Meeting of Amateurs, of Toulouse. Described by M.* Dispan, *Professor of Chemistry in the Institution of that City*.*

For several years past, plenty of experiments have been published on the effects of the gaseous oxide of azote inhaled into the lungs. But these experiments, almost always

* From *Annales de Chimie*, tome lvi.

different, and often contradictory, in their results, present no basis positive enough on which to ground an opinion on the subject. Such, at least, were the motives which induced several distinguished chemical amateurs of Toulouse to judge of the singular properties attributed to this species of gas, by actual experiments made on themselves. As I can answer for the purity of the substances employed, and the general precautions used on the occasion; and having also taken minutes on the spot of the effects of the gas on twelve persons at least, several of whom repeated it two or three times; I presume that this publication will be read with some interest.

First Meeting.

The nitrate of ammonia made use of was confusedly crystallized, but nevertheless very neutral. The taste was pungent, and it had a slight smell. It had been wholly formed by the distillation of sal-ammoniac, from common potash, and the simultaneous saturation of pure nitric acid by the ammoniacal gas liberated by the above process.

We then put about a hectogramme (nearly two ounces) of this salt in a small retort, and placed it in a sand-bath. The salt melted and boiled some time before emitting any gas: at last the retort was filled with a white vapour which soon disappeared, and the gas immediately began to be liberated very rapidly; when we filled several bladders with it. In a short time the production of the gas ceased, and when we stopped the operation, almost nothing remained in the retort; which convinced us that no accident had happened.

Emboldened by this result, we put into a retort nearly three hectogrammes (about six ounces) of the same salt, which yielded enough of gas to fill seven or eight bladders, although we lost a great part of it. The operation was continued equally successfully as at first, until nothing more remained in the retort; but a circumstance occurred which surprised us all, and for which we could not account: this was the formation of an abundant red vapour in the inside of the retort in proportion as it cooled, although the last gas contained no nitrous gas: this we ascertained by suitable experiments.

Effects

Effects of the Gaseous Oxide of Azote when introduced into the Lungs by Respiration.

All those who have tasted or inhaled this gas agree that it has a taste strongly saccharine, the impression of which has been often retained during the whole day. I myself experienced also a nitric taste (in truth, it was the gas latterly produced which I tasted). M. de M—— said, no doubt upon perceiving the same taste, that there was something styptic in it. The rest did not perceive any thing else than a saccharine taste; which is certainly a very decisive one, considering the small quantity of matter which the gas could contain.

The following is a precise account of the different effects of the gas, as they were successively experienced by the various gentlemen who inhaled it. The gas was inhaled by means of a bladder with a stop-tock, the nostrils being held close, and the lungs emptied as much as possible.

M. G—— suddenly lost all recollection at the third inhalation: he continued it for five minutes; after which he returned to his senses very much fatigued, without being able to recollect any other sensation than a sudden fainting, and a tingling in the temples.

M. de M—— experienced a saccharine and styptic taste, a great dilatation, accompanied with a heat in the breast; his veins were swelled, and his pulse fell. Every object appeared to dance round before his eyes. He thought, however, that he could have supported a stronger dose. The bladder was not large enough for his lungs.

M. de P—— experienced a saccharine taste at the first inhalation, which became afterwards imperceptible. His lungs were strongly dilated, and with great heat. He experienced very agreeable sensations after laying aside the bladder, and he fell into involuntary fits of laughter.

M. de S—— experienced the same saccharine taste as the former gentlemen, and the impression of it continued from ten o'clock in the morning until past midnight; he also experienced vertigoes, and his legs remained benumbed all day.

M. G——,

M. G——, the same saccharine taste. After laying aside the bladder he experienced a dimness of sight, and afterwards a very pleasant sensation, which spread through his whole body. His legs were benumbed.

M. de C——, a saccharine taste during the whole day, tingling in his ears, legs benumbed, the stomach almost choked up. Upon the whole, he regarded what he had experienced as more painful than agreeable.

Experiment.

I was anxious to know in what degree the necessary constraint of breathing in a bladder influenced the above results. These gentlemen, at my request, put themselves to the trouble of breathing common air in the same manner. They only found themselves mechanically fatigued; and all of them agreed in the same results.

Another Experiment.

I was also anxious to try the effect of oxygen gas. Those who inhaled it assured me that they experienced little difference between it and common air, which consisted solely of an augmentation of heat in the lungs.

Conclusion.

Thus the singular effects above described belong to the gaseous oxide of azote alone.

Second Meeting.

The object of our second meeting was to repeat more at large the experiments relative to the respirability of the gaseous oxide of azote.

We put into a retort about eight hectogrammes (nearly 16 ounces) of the nitrate of ammonia prepared as at first; a lengthening tube was adapted to a bottle with two necks, whence, by means of a tube of Welther, the gas proceeds into the tub. The retort rested on a sand-bath.

Upon the first application of the heat the salt melted, and almost at the same time some reddish vapours were formed in the retort, but in very small quantity. The air of the vessels

vessels which the heat disengaged yielded also a nitrous smell to such a degree, that we were afraid of the success of the operation; these vapours and smells, however, insensibly diminished, and at last totally disappeared. About this stage of the operation, the bubbles which were disengaged had a manifest smell of the prussic acid, which continued a long time. At length the retort was filled with white vapours, and the gaseous oxide of azote began to pass, which it soon did so abundantly that we removed the fire; and having again replaced the charcoal, the gas, which had ceased during the interval, reappeared in such abundance, that the luting gave way at one place. In spite of the loss of a considerable quantity in consequence of this accident, the disengagement of the gas into the tub continued very rapidly during a quarter of an hour.

This circumstance gave us reason to believe, that if the luting had not given way, there certainly would have been an explosion.

Twelve persons submitted themselves to the experiment of inhaling the gas at this meeting, several of whom repeated it twice. It is right to observe, that the most of them had inhaled the gas at the last meeting, when two out of seven experienced a sensation of pleasure; but on this occasion none at all, and not even these two, experienced such a sensation. On the contrary, several suffered very severely.

M. de M—— stamped with his feet all the time he held the bladder. After recovering from a profound stupor, he informed us, that he felt as if he had got a blow with a dagger on the back part of his head capable of killing an ox, and which he would not experience again for any thing in the world. The rest, in general, experienced vertigoes and dazzling of their eyes, which were succeeded in some of them by fits of laughter. I myself was of the latter number, and the following is an exact account of what I felt:

At the first inspiration I emptied the bladder. A saccharine taste immediately filled my mouth and my lungs entirely, which dilated considerably. I emptied my lungs and filled them again; but at the third trial my ears tingled, and

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I let go the bladder. I continued an instant, without losing my recollection, rolling my eyes in a dumb stupor; I then burst into such a fit of laughter as I never experienced before in my life. After some seconds, this tendency to laugh ceased, as did also the other symptoms.

M. de P—— experienced no other effect than a convulsive motion in some muscles of the face. But he had a violent diarrhœa in the course of the day; and M. D—— experienced the same effects.

Upon the whole, it would be very difficult to ascertain the effects of gaseous oxide of azote in an exact and general manner, since these effects vary in different individuals, and, what is very singular, even in the same individual. M. de S——, who inhaled it four times, felt new impressions every time. For my own part, I only experienced a tendency to laughter in one of the several times I inhaled it. I should certainly have fainted had I pushed the experiment further.

Effects of the Gaseous Oxide of Azote upon Animals.

I have only one experiment upon this subject, but it appears worth reporting.

I put a greenfinch into a glass vessel full of gaseous oxide of azote. The bird appeared to suffer nothing at first; but it soon closed its eyes, laid itself gently on its side, and appeared as if asleep. When brought into the open air and set at liberty, it placed itself on its legs, but did not attempt to fly away. It was submitted an hour afterwards to a second experiment, and allowed to remain a little longer; but no efforts could restore it to life.

It appears very remarkable that this bird made no effort to get away, and that it felt no convulsions, as generally takes place in the other gases.

XL. *Re-*

XL. *Report of Cases in the Finsbury Dispensary, from the 1st of January to the 31st of March 1806. By* JOHN TAUNTON, *Esq. Surgeon to the City and Finsbury Dispensaries, and Lecturer on Anatomy, Physiology, and Surgery.*

SINCE last report (Phil. Mag. vol. xxiii. p. 312.) there have been admitted into this dispensary 160 patients.

Cured - - -	66
Relieved - -	5
Irregular - -	3
Under cure - -	86
	160

Of these, 33 have been visited at home, and eight have submitted to operations.

In the last surgical report (see January) there were 80 patients under cure, 70 of whom have been cured and 10 relieved, 18 have been home patients, and two have undergone operations.

Mrs. Cuffee, æt. 50, has been subject to rheumatism and asthma for several years. She observed a tumour in her left breast some years ago, irregular on its surface and occasionally attended with darting pain, which was not severe, so as to excite much attention, till March 1805, when the breast had increased much in size, and was become very painful. The unfavourable symptoms continuing, she was admitted into the dispensary in November last; but, owing to her bad state of health, nothing could be done at that time to remove the disease in her breast. In January, her health being improved, it was determined, in consultation, to propose the operation, that being the only mode of treatment likely to give her any chance for recovery. To this she readily consented, and requested that an early day might be named for its performance.

The operation was performed on the 28th of January, which she underwent with the greatest fortitude, and had very little symptomatic fever considering its magnitude: as she had naturally a very full breast, the size of which was much increased by the disease, the wound filled with granulations,

lations, and was completely cicatrized in much less time than could have been expected from its size, as the incision was 14 inches in length, and a large portion of the integuments were necessarily removed.

Mrs. F., æt. 31, has had several children, and always enjoyed good health previous to the present disease. About the end of May she observed a tumour on the lower part of the neck, but, as it was not attended with much pain, it did not excite much attention: from the beginning it appeared to be very hard and immovable, and, as it increased in size, it impeded respiration and deglutition.

She was admitted into the dispensary on the 21st of September, when the tumour was become very painful both in deglutition and respiration, and, from its rapid enlargement, had much alarmed the patient. It was found to be an exostosis of the clavicle, near its connection with the sternum. It reached to the upper part of the larynx, and its base where it grew from the clavicle was more than three inches in length, and it must have been more than nine inches in circumference. In this case little was to be expected from any mode of treatment short of an operation; which must have been attended with great danger, as the tumour grew from the inner as well as from the upper surface of the clavicle, and was nearly in contact with the carotid artery.

Cicuta fomentations were ordered with a view to alleviate the pain; in which they happily succeeded, and appeared to arrest the growth of the tumour. She took the infusion of quassia, and the fomentations were continued till the 17th of October, when it was evident that the tumour had not increased, and the pain was much lessened during the above period. The emp. hydrarg. cum ammoniac. was applied, with a view to increase the action of the absorbents: some magnesia vitriolata was taken occasionally, and the plaster renewed once in about every ten or twelve days: the tumour gradually decreased in size, was entirely discussed by the end of December, and she was discharged cured on the 6th of January 1806.

Greville-street, Hatton-garden,
April 5, 1806.

XLI. *On*

XLI. *On the Oxidation of Metals in general, and particularly the Oxidation of Iron. Read in the French National Institute by M.* THENARD*.

As soon as oxygen was discovered, researches became general to ascertain its properties, and it was very soon discovered that this gas was the universal agent of combustion. Phlogiston was immediately exploded, and hypotheses totally contrary to experience were no longer resorted to in order to explain the generality of phænomena. By admitting the presence of this principle, more or less, in the metallic calces, an exact account may be taken of the augmentation of weight which metals receive when calcined. But, however simple this theory may appear at present, it was nevertheless the result of a grand effort of genius.

When it was clearly demonstrated that metals, as well as other bodies, so far from losing their principles by combustion, absorbed a new one, because they are thereby augmented in weight; when Lavoisier taught us, that in this phænomenon, the cause of which was for such a long time unknown, the atmosphere was decomposed, and that one of its constituents formed a new combination, the properties of burnt bodies were examined with more care; a great number of new principles were discovered in such bodies, and in many of them the quantities of oxygen and of the radicals which formed them were determined. These new observations were again the source of many discoveries. It was seen that the same combustible body might be combined with oxygen in different quantities, and that consequently several oxides, as well as several acids, might have the same radical. Frequent applications of this principle are met with, particularly in the oxidation of metals; and at this period, lead, antimony, and manganese, offer the most remarkable of these applications. It was this variety of oxides which led the author of the Chemical Statics to think that there actually was not so much difference as had been previously believed between oxides of the same genus; and

* From *Annales de Chimie*, tome lvi.

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this led him to believe, supported also by reflections which were the result of many experiments, that probably even metals pass from the metallic state to the maximum of oxygenation by going through all the intermediate degrees of oxidation in such a manner, that for each metal there is a multitude of different oxides.

I am well persuaded that the number of metallic oxides is much greater than most chemists admit, and that they do not pass immediately, as is supposed, at least with respect to some of them, from a weak degree of oxygenation to a very high one; that between these last there exists one or more intermediate degrees which constitute as many particular oxides: but I confess that I am not yet convinced that there are as many oxides as possible degrees of oxygenation; and if the theory admits them, experience seems to reject them: in fact, why should not these different oxides combine with acids? The cause cannot be attributed to any thing else than that metals, at a certain degree of oxidation, have more affinity for acids than at any other; but for that very reason it may happen, that, when combined with a given quantity of oxygen, they may form fixed oxides, while with a greater or less quantity these oxides can only have a momentary existence. The latter would therefore be placed between the former; there would be as many degrees through which the former would be obliged to pass without being able to stop; and besides, this is exactly what we observe of acids which have the same radical. Will it be said, that between the *sulphurous* acid and *sulphuric* acid, between the *phosphorous* and *phosphoric* acids, there are several intermediate acids? And if these intermediate acids do not exist; if sulphur, phosphorus, &c. cannot form any more than two acids, why do metals give birth to such a multitude of oxides? why should not there be fixed degrees of oxygenation as well for the one as the other? In short, how could it happen that hydrogen forms only one oxide, although susceptible of fixing more than five times and a half its weight of oxygen? There are many reasons which incline us to believe that metals are absolutely like other combustible bodies; and although it is proved that the latter cannot form, with the general

neral principle of combustion, a multitude of different combinations, I am persuaded that metals, which we regard with reason as analogous bodies, are not more susceptible than the others of a multitude of different degrees of oxidation.

If we are permitted to entertain doubts on this view of the subject, we may certainly have some upon the nature of the oxides in their combinations with acids; and even if this truth had not been long admitted by chemists, some general observations would suffice to place the subject in a clear point of view. Let us take a glance, however, of each of these oxides, and attentively consider the oxides of iron, which are the principal objects of this memoir. Here every thing demonstrates, that in combinations of this sort the oxides are constant. Although cobalt, nickel, lead, zinc, gold, and platina, are all the bases of several oxides, yet in all the salts which they form they are always equally oxidated: thus the oxide is blue in all the salts of cobalt, green in those of nickel; it is white in those of bismuth, zinc, and lead; and it is gray in those of silver, yellow in those of gold, and brown in those of platina: it varies, to be sure, in the salts of antimony, tin, mercury, copper, and iron; but still with several of them it is only certain of their oxides that can unite with acids. Two white oxides of antimony alone are susceptible of this combination—the white volatile oxide and the white oxide of the second degree; the white oxide at the maximum is not attackable, except by the muriatic acid: still, however, if it is not in a state of very minute division, the muriatic acid dissolves it with great difficulty, and always by partly passing to the state of oxygenated muriatic acid. Tin, mercury, and copper, like antimony, do not form saline combinations with the acids, but under two states of oxidation; tin in the state of a gray and a white oxide, mercury in the state of a black and a red oxide, and copper in the state of a yellowish white and a brown oxide.

Hitherto it was thought that it was the same case with iron, and that in all the salts which it was susceptible of forming, the oxide was always green or red. Some chemists, however, have admitted an intermediate oxide: they believed

that there was also a yellow oxide, because ferruginous salts present themselves sometimes under this colour. Reasoning upon the hypothesis that all the metallic salts were always of the same shade as their oxides, they were necessarily led to adopt that idea; but it is now very well known that this method of viewing the subject is often erroneous, and that a salt is often white or red although its oxide is red or blue. Another salt, consequently, might be yellow, and have a red oxide for its base: this is exactly the case with all the yellow salts of iron. Besides the green and red oxide, however, there exists another oxide of iron which performs a very important part in the ferruginous saline combinations; and as, on the one hand, the formation of several productions manufactured in the workshops, and, on the other hand, as the explanation of several phænomena, which often present themselves in the arts, depend upon the existence of this oxide, I think it my duty to enter into some details respecting it.

It is formed when iron is treated with most of the acids; and we obtain it by decomposing by means of potash, soda, and ammonia, the various salts so formed. Above all, by means of a solution of iron in sulphuric acid we may easily demonstrate its existence. If in this recent solution we pour some alkali, a white precipitate is formed, which becomes speedily green at the surface and soon passes to a red colour *. These changes of colour, which take place in the whole mass if it is agitated, are evidently produced by an absorption of oxygen; because, in repeating the experiment in a flask, the air which it contains sensibly diminishes in volume, and soon extinguishes a taper plunged into it, when the white oxide has become green, and with greater reason when it has passed to the red.

It is this white oxide, hypersaturated with sulphuric acid, which forms, in a great measure, the sulphate of iron used

* I poured some hyperoxidated sulphate of iron into a great excess of caustic potash, and I always observed a white precipitate. I even boiled the mixture in a retort the neck of which was plunged into water; the oxide became green, and was even red at the surface; but the lower part was always perceptibly white, although it contained no sulphuric acid. Of this I convinced myself by washing it, dissolving it in the muriatic acid, and by adding nitrate of barytes to it.

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in commerce. Nevertheless, besides this combination, the white oxide may still form a sulphate much more acid; and then, instead of being of a dark green, the compound approaches to a clear emerald green. Thus we find there is a green oxide as well as a red oxide of iron; each of them, on being united with sulphuric acid, gives birth to at least two very distinct salts. Let us now proceed to examine these six kinds of sulphate of iron.

I shall distinguish the first of them by the names of acidulated sulphate and acid sulphate of white iron, because the oxide in these is colourless, and the one much more acid than the other; the second, for the same reason, under the names of acidulated sulphate and acid sulphate of green iron; and the third, for the same reason, I shall term neutral and acid sulphates of red iron.

The acidulated sulphate of white iron is obtained by boiling sulphuric acid diluted with water upon an excess of iron filings or turnings. When dissolved, or in particular crystallized, it is always of a bottle green, and the more coloured it is, the more is it esteemed in commerce. It instantly loses this colour upon an addition of sulphuric acid; it then takes an emerald green and becomes acid sulphate of white iron, which alters blue colours much more than the former*: it is then less fit for the operations of the arts, and rejected by manufacturers, who give a preference to the former, not from prejudice, but founded on the nature of saline

* When sulphuric acid is poured into a solution of acidulated sulphate of white iron, evaporated in such a manner as to mark 36° to the weight of the liquor, it almost immediately forms an abundant white and crystalline precipitate, which is nothing else but acid sulphate of white iron. This is the reason why, in vitriol manufactories, it sometimes happens that the liquor becomes all at once troubled to a certain degree, and leaves a deposit of a white matter, which is rejected by the manufacturer, and gets the name of magnesia. The solution is then too acid, and the escape of this excess of acid takes place instantly, so as to form an acid sulphate which is precipitated, and an acidulated sulphate which remains in solution and crystallizes much more regularly than the first. This inconvenience might be guarded against by boiling the solution a longer time with iron, and adding water to it, if necessary; because the acidulated sulphate of white iron or the sulphate of iron of commerce, when it is too much evaporated, is also susceptible of partly concreting itself; at least it sometimes does so suddenly, all in a mass.

compositions; it may be transformed also into acidulated sulphate by heating it with iron; this is what is done in several vitriol manufactories, and particularly at Beauvais, where this salt is extracted from pyritous turf, which is burned, and which without that would yield acid sulphate, less beneficial in commerce. This may be done with every kind of acid sulphate, whatever is its origin; whether natural or artificial, the transformation always takes place: it would still be made whether the oxide of iron was green or red. The acidulated and acid sulphates of white iron are both precipitated white by the alkalies; they instantly decompose oxygenated muriatic acid, and pass, in proportion as more or less of it is introduced into them, to that state of sulphate in which the oxide is green or red; it is in this manner that the oxygenated muriatic acid acts upon the pure white oxide; such also is the action of the air upon the combinations of this oxide with acids, and particularly with sulphuric acid. This is the reason why the colour of these solutions is not constant; from the green they pass to the red; the liquor is troubled, deposits a yellow matter, and then ceases to be coloured. All these phænomena may be explained from natural causes, and are the consequences of the properties which the other sulphates of iron present to us, of which we are now about to speak.

The acidulated and acid sulphates of green iron, both of which result from the combination of the green oxide of iron with sulphuric acid, present the most striking differences. The first (being that which is only a little acid) does not crystallize at all, not being able to exist except in a liquid state; if submitted to evaporation it absorbs oxygen from the air, becomes troubled, and deposits neutral sulphate, yellow, insoluble, and very much oxidated: thus it transforms itself into acid sulphate, in which the oxide is always green, but itself almost colourless, and which resists every kind of decomposition much more than the other: although it has green oxide for its base, it is red itself: this is what occasions the error of most chemists, who, down to the present moment, have regarded it as a sulphate very much oxygenated: thence it again happens that the acidulated

lated sulphate of white iron, the solution of which is of a fine green, reddens on its exposure to the air.

Oxygenated muriatic acid converts it all at once into acid sulphate highly oxidated; iron, on the contrary, converts it into acidulated green or little oxidated sulphate; the sulphuric acid makes it lose its red colour immediately, by transforming it into an acid sulphate almost colourless, or at least only slightly green. This acid sulphate may be crystallized by a well managed evaporation; the crystals formed are something of an emerald green colour, and in this respect it approaches an acid sulphate little oxidated; they do not sparkle nor deliquesce; their solution in water, which is but little coloured, does not, like the acid sulphate of white iron, absorb oxygen till after a long time: the excess of acid which they contain fixes to a certain degree the oxide of iron; nevertheless by means of oxygenated muriatic acid the oxide becomes red; and by means of iron it passes to the state of white oxide, particularly at the temperature of boiling water. Both the acidulated and acid sulphates of green iron precipitate themselves green by means of the alkalis. The precipitate contains no acid when a great excess of base is added, and particularly when heated: in every instance it is green. It is by treating the red oxide of iron with diluted sulphuric acid that we obtain the acid sulphate of red iron; the solution would not take place if the acid were concentrated. The acid sulphate of red iron contains more acid in excess than the acid sulphate of green iron, and the latter much more than the acid sulphate little oxidated. These properties are common to them with all oxides of the same kind; they require the less of any acid for their saturation the less oxygen they contain; and, on the contrary, they require the more acid the more they are oxygenated; this is proved by antimony, mercury, tin, copper, and iron. They alone among all the metals combine with acids at different degrees of oxygenation; and all, without exception, are subject to this law. This is the reason why the solution of muriate of tin little oxidated becomes troubled by the contact of the air, or when being crystallized we attempt to dissolve it. It is also for this reason that this

same recent solution of tin entirely decomposes corrosive sublimate and revives the mercury, not only taking the oxygen from it, but even the muriatic acid. Thence it also happens that the acid sulphate of white iron transforms itself in part, by absorbing oxygen from the atmosphere, into green acidulated sulphate, and assumes a slight red tint; thence it happens at last, that this same salt, when it is only acidulated, becomes troubled all at once upon pouring aërated water into its solution: several other phænomena depend upon the same cause; but, should I insist longer on this branch, I should wander too far from the subject on which I propose to treat.

Like the two other acid sulphates, the red acid sulphate of iron is almost colourless; it takes a very strong colour, and reddens when its excess of acid is saturated in part by potash; by adding a greater quantity of a salifiable base, a neutral sulphate is precipitated from it, which is, however, susceptible of being decomposed by the alkalis: it separates, as well as all the highly oxygenated salts of iron, sulphur from hydrogen, and returns to the state of a very acid green or white sulphate of iron; it yields no crystals upon evaporation: the iron changes it into a less oxygenated sulphate.

I have but little to say of the highly oxidated neutral sulphate: it is yellow and insoluble; this is the sulphate which in time, by exposure to the air, is precipitated from solutions of green or white acidulated sulphate: this is also the sulphate which is deposited when the solution of green acidulated sulphate of iron is evaporated, and which from being red becomes almost colourless, because then it passes to the state of acid sulphate; it is this salt, in short, which chemists formerly took for a particular oxide, and which induced them to admit a yellow oxide of iron, as intermediate between the red and the green.

The action of the muriatic and nitric acids upon iron resembles much that of the sulphuric acid; that of the muriatic acid does not differ at all, but the acid muriates of iron are not so well characterized as the acid sulphates; the muriate when little oxidated crystallizes very well, and the crystals formed from it are of a fine green; but when the solution

tion is exposed to the air, the iron becomes more oxidated, and as it then requires a greater quantity of muriatic acid to dissolve it, it is partly precipitated. No nitrate, however, exists in which the oxide is white. We can only obtain green and red nitrates of iron; the first is formed by taking five parts of the acid, and the second by employing twelve or fifteen parts of acid. If the acid was more concentrated, a portion of the red oxide would be precipitated, and very little of it would be found in solution, if it contained from thirty-six to forty parts. This precipitation would not take place, unless the red oxide was very little divided: it is to this cause that we ought to attribute the inaction of the nitric acid upon colcothar, as well as the little action which sulphuric acid itself has upon this substance. What proves this is the property which these two acids have of easily dissolving the red oxide in the gelatinous state, or recently precipitated from the nitrate or muriate by alkalis.

We find in the other acids the same mode of action as in the sulphuric acid; but the generality of salts which result from them, being insoluble, are rather obtained by means of double decompositions than directly. I shall not here take notice of all the saline bodies, on account of the little interest they have hitherto excited in the arts and sciences; I shall only examine the two most important,—the gallates, which serve as the bases of the black dye: and the prussiates, the use of which has so much increased for these thirty years past.

We know that the gallic acid attacks iron even at the temperature of the atmosphere; that it dissolves it with the disengagement of that proportion of hydrogen gas necessary when water is decomposed; that this solution, at that time colourless, becomes blue very soon on its exposure to the air; and that then it begins to be thick, and passes to a blackish gray. All these phænomena have been described with much care by M. Proust, but to this moment the whole have not received a satisfactory explanation. I discovered the cause of their difference in the three oxides which iron is susceptible of forming. Thus, by pouring gallic acid into acidulated sulphate of white iron, no precipitate is obtained;

with the acidulated and acid sulphate of green iron it forms a precipitate of a fine blue; and by repeating the experiment with a highly oxygenated salt of iron, the matter deposited is of a black inclining to gray. An excess of acid, however, hinders the deposition from taking place. This is what we remark in the acidulated sulphate of white iron, where the addition of an alkali is indispensable for the formation of the gallate which then presents itself under the form of violet-coloured flakes; this is also what is observed in red acid sulphate of iron, where the saturation of the acid is necessary for the precipitation of the composition; this, on the contrary, does not take place in the red muriate of iron which is only a little acid.

The three oxides of iron of which we have been speaking, and all of which may be combined with acids, form combinations with the prussic acid much more multiplied than those we are about to examine. Not only do prussiates of iron exist, neutral and with an excess of oxide, but both the one and the other are susceptible of uniting with the prussiate of potash and forming triple insoluble salts, if the metallic prussiate predominates; and, on the contrary, it will form soluble salts when there is almost nothing in it but alkaline prussiate. Such, in few words, is the general history of Prussian blue; but it is of too much importance to science not to consider particularly every one of its component parts.

One department of this history, the most useful to study, is the difference of colour in the precipitates obtained by decomposing solutions of iron by the alkaline prussiates: the shades of these precipitates are singularly various. Sometimes they are white, and sometimes greenish; but oftenest they are more or less blue, and the eye accustomed to judge of colours recognises many different shades in these principal colours. These effects do not depend entirely upon the state of the oxidation of the iron; they depend also upon the state of the alkaline prussiate and the metallic solution.

If the iron is little oxidated, the solution but little acid, and the prussiate has an excess of alkali, a white precipitate will be obtained; it will be greenish white, if, while the

other

other circumstances are the same, the prussiate is neutral; the one differs from the other only in the proportion of their constituent principles; the former contains an excess of oxide, while in the latter the acid is merely saturated with it. Thus the acids instantly render the prussiate of white iron greenish, by taking from it a part of its base; and the alkalis make the slightly green prussiate of iron pass to a white colour, by attracting to themselves a part of the acid which enters into its composition: thus by art we may transform these two salts, the one into the other at pleasure, merely by varying the quantity of the substances which form them.

These precipitates are not simple prussiates of iron, as has been believed for a long time; they contain besides prussiate of potash, as Berthollet has proved. The prussiate of potash has even so much affinity for the prussiate of iron, that the sulphuric acid only decomposes in part the insoluble combination which these two salts form together. If a slight excess of acid only is added, the residue still contains plenty of potash, and is perceptibly green; it becomes greenish blue by a greater quantity of acid, and, when analysed, little alkali and oxide is found in it: this attraction is still more striking when the prussiate of potash is abundant enough to render the combination soluble; then the sulphuric acid does not disengage from it even the most trifling smell of bitter almonds, and the sulphate of iron uniformly produces a very great quantity of precipitate; while, if the alkaline salt is pure, the carbonic acid is powerful enough to disengage the prussic acid from it.

The same results are obtained by substituting, in place of the acidulated sulphate of iron not much oxidated, the green acidulated sulphate of iron and the red muriate of iron which is only little acid: its precipitates are always triple prussiates of iron and potash; that one the oxide of which is green, is less blue than the one the oxide of which is red; both change colour if united with a great excess of oxide; the first becomes yellowish white, and the second reddish yellow; if the excess of oxide is less, they approach nearer to a blue colour; but both the one and the other are susceptible of being brightened by the acids.

Six

Six prussiates of iron therefore exist, which are well characterized; three of them are neutral, and three with an excess of oxide; the excess of oxide may be more or less considerable, which varies in a singular manner the colour of these prussiates. These six prussiates of iron may be combined with different quantities of prussiate of potash, and without doubt also with the other alkaline prussiates, in such a manner that this genus of salts is very numerous in species. It is generally observed that those which contain much alkaline prussiate are soluble; those, on the contrary, which contain much prussiate of iron are insoluble. We know that Prussian blue becomes green by exposure to the air, and that it then forms red oxygenated prussiate of iron. When the ordinary prussiate of potash is boiled with Prussian blue, which is thus oxygenated in whole or in part, the oxygenated prussic acid acts upon the potash, and the prussic acid combined with it acts upon the oxide of iron in such a manner, that the filtered liquor precipitates the red nitrate or muriate of iron of a green colour. This is the reason why certain solutions of prussiates, made by means of Prussian blue, precipitate the sulphate of iron little oxidated in a white colour instead of a blue one.

It is this variety presented by prussiates in their composition, which renders their preparation so difficult. This, however, is not the only obstacle we meet with in the manufacture of Prussian blue; there are still a great many others, which were only surmounted after a long time, and some of them even exist at this moment. One of the perfections necessary to be attempted in this art, would be to save for other uses the great quantity of carbonate of ammonia formed by calcination, which would admit of the prussiate of iron being used with much more advantage in commerce. I am convinced, by experiments made with great care, that on calcining potash with animal matters, as much carbonate of ammonia and prussiate of potash is obtained as when such animal matters are distilled by themselves; so that as much prussiate of iron is formed with the carbon resulting from their distillation, as with all their principles united. We may thus make sal-ammoniac at the same

same time with Prussian blue, and consequently double the products without much augmenting the expense of the manufacturer. The manufacturers of Prussian blue ought not only to direct their attention to this point, but also to several others which might improve their different processes. Some do not employ the most advantageous proportions of potash and blood, some do not add iron, and several maintain the heat too long: only a very few of them take the trouble of crystallizing the prussiate of potash; many of them do not know that in the calcination of substances the sulphate of potash contained in the potash made use of is transformed into sulphurets, and for a much stronger reason they are ignorant of the means of preventing the bad effects of it. In short, almost the whole of them consecrate too much time in oxygenating Prussian blue; by that means augment the manual labour, and deprive themselves of the power of making more considerable quantities of it. The prosperity of their manufactories depends, however, on all these considerations united.

The following is what I have remarked on this point:

Experience has proved to me, that a quantity of potash equal to that of blood was preferable to every other proportion; we know that the addition of iron favours the formation of prussiate of potash, and fixes it. It is not less evident that the effect of fire is also as important to observe, because, by exceeding the point of fusion, we decompose the prussiate of potash, or the matter which ought to form it. It is equally necessary to crystallize the prussiate of potash, particularly when we wish to obtain Prussian blue of a superior quality. On the one hand, we thus transform the sulphurated hydro-sulphuret of potash into sulphate of potash; and, on the other hand, it is only necessary to add very little alum in order to saturate the excess of alkali: we might again decompose the sulphurated hydro-sulphuret of potash by means of the sulphuric acid, because the acid does not alter the prussiate of potash combined with a certain quantity of prussiate of iron. This crystallization is not indispensable in the fabrication of common Prussian blue, into which much alumine enters, which cannot be separated

but

but by a great quantity of potash; it is sufficient to boil the liquor some time in order to burn the hydro-sulphuret; and, besides, the sulphurated hydrogen is separated by the acid of the alum. In short, the operation would be very much abridged, if, instead of washing the prussiate of iron in a great quantity of water, it was only mixed with a small quantity of oxygenated muriate of lime.

Such are the observations which I considered it my duty to collect in the present memoir; the whole facts do not belong to my subject. In order to support those which do, I have connected with them some observations which were the result of the labours of different chemists. Sometimes I have even repeated facts known long ago, in order to present at one view the properties of a body, or the track we ought to follow in an operation. Yet still, I believe, I have examined, with some advantage to the arts and sciences, the oxidation of iron and the combinations of its oxides with acids. The existence of the white oxide of iron will make us acquainted with various phænomena the causes of which were hitherto unknown; it explains, in particular, that variety of colours which all the ferruginous salts present to us. It spreads some light on one of the finest and most important dyes known, viz. that of black. It does not throw less light on the manufacture of Prussian blue, upon the improvement of which much yet remains to be done. In short, it deeply concerns the art of obtaining sulphate of iron, the perfection of which becomes daily more and more desirable.

XLII. *On the Combination of Antimony with Tin.* By M. Thenard *.

I was led, about a year ago, to make the observations I am now about to communicate, upon examining an alloy attempted to be introduced into commerce, and to which such miraculous properties were improperly attributed, that, according to some accounts, it might have been substituted in place of silver.

* From *Annales de Chimie*, tome lv.

It

It was said to be very malleable; but, above all, its admirers boasted of its inalterability. By its appearance I thought that it contained plenty of tin; and as the price of it was moderate, I presumed it also contained antimony, and, to a certain degree, zinc or lead also. This was the reason why I treated 100 parts of it with the nitric acid. This portion of it was immediately attacked by a violent effervescence, and converted into a white powder. At the end of half an hour's ebullition, after having filtered the liquor, I tried it successively by the potash of commerce, the sulphuric acid, and the hydro-sulphurets.

All these re-agents having indicated that it contained nothing metallic in solution, I thought it very probable that this alloy was composed of tin and antimony alone. In order to ascertain this completely, I took the above white powder produced by the nitric acid, and I dissolved it in the muriatic acid. I concentrated the solution, and diluted it with water. It produced, as I had previously conjectured, a very abundant precipitate; but having allowed the liquor to settle a whole day, and having decanted it, I found no more metallic traces than in the preceding. Ammonia scarcely troubled it at all, and the hydro-sulphuret of potash gave it a slight yellow colour. Although I had performed the operation with much care, I could not at first give credit to this result, it appeared so singular, and it was only by repeating it that I found I was not deceived. What ought I to conclude from this? Had I consulted only the known properties of the oxides of antimony and tin, I should have been led to believe that the substance I had examined was nothing but antimony. It was however sensibly malleable, and must therefore have contained another metal. Every thing led me to believe that this was tin; and, in fact, I actually formed an alloy with antimony and tin, which enjoyed all the properties of the one above described. Four parts of tin and one of antimony give a very ductile alloy. Equal parts of each also give an alloy which possesses still a certain ductility. But if a few centiemes of lead enter into the tin, both the above alloys become very brittle. The intermediate alloys possess properties relative to the quantities

of tin and antimony which constitute them. All of them are not precipitated from their solution in the nitro-muriatic acid by means of water. There are, necessarily, limits; but these limits are very distant. It is necessary for the alloy to contain one-third of its weight of antimony to have this property, particularly if the excess of acid has been in a great measure driven off by evaporation. I ought, however, to notice that the precipitation is not completely made in less than 24 hours, when antimony predominates; because then the latter portions, which are a combination of the two oxides with the muriatic acid, only separate by little and little.

This is not, however, the only example we have of combinations of oxides. We cannot doubt that the oxide of tin does not combine with the oxide of lead; for on calcining an alloy of three or four parts of lead and one part of tin, it soon burns in the manner of a pyrophorus, and is converted all at once into oxide; while tin, much more combustible than lead, far from presenting this sort of phænomenon, does not transform itself into putty but after a long time, even by multiplying its points of contact with the air. I made several other trials in order to ascertain if the oxide of antimony would act upon other oxides as well as upon the oxide of tin. I think I ascertained that the latter is the only one which the antimonial oxide takes with it in precipitation, and that the oxide of bismuth does not precipitate any oxide along with itself, not even the oxide of tin.

After having thus proved synthetically that my alloy was formed of tin and antimony, I proceeded to inquire after the analytical means proper to produce the separation of these two constituent principles. I then employed the muriatic acid, which dissolves tin very well, and does not attack the oxide of antimony. It had hardly any action upon the alloy; and, besides, in the portion dissolved I found antimony. I next tried sulphurated hydrogen. I knew that it would easily precipitate the muriate of antimony; and that, on the contrary, it would not decompose but with difficulty the highly oxidated muriate of tin. This method had no more success than the former.

The

The liquor concreted in a mass, and did not admit of my obtaining the salt of tin by itself; and even the separation was far from being exact. I tried also, but always without effect, to volatilize the antimony, by heating the alloy strongly in close vessels. Finally, I treated the alloy with nitro-muriatic acid, and I did it in such a manner that the two muriates were highly oxidated. I distilled them in a retort, taking care to agitate it incessantly in order to avoid the boiling over, particularly at the end of the operation. I urged the fire even until the bottom of the retort was red, and I obtained sensibly all the muriate of tin. The muriate of antimony, which when highly oxidated is not volatile, remained in the retort. A very small quantity of this salt alone had passed into the receiver. The muriate of tin also is scarcely troubled by means of water. Although this process was perhaps not very rigorous, in order to separate the two oxides of tin and antimony, I look upon it as the best hitherto employed, and I consider it good enough to indicate how many centiemes of tin are contained in antimony, or *vice versâ*.

The consequences which may be drawn from this new fact, important enough to merit the attention of chemists, are evident. Is it well ascertained that the mines of antimony do not contain tin, and *vice versâ*? Certainly not, because the methods of analysis hitherto practised to separate these two metals have been inexact. Already have I searched for tin in sulphuret of antimony, and I never discovered a single trace of it; but I have not yet analysed any other ores of antimony, nor of tin. It is a labour which appears to me very useful to make, and which perhaps would furnish interesting results.

XLIII. *Abridg-*

XLIII. *Abridgment of certain Papers written on the apparent Magnitude of the horizontal Moon, and published at sundry Times*. To which are added new Experiments to prove the Truth of the Author's Theory, and to exhibit a clear Representation of the Phænomenon on optical Principles. By* Ez. Walker, *Esq.*

To Mr. Tilloch.

Lynn, March 11, 1806.

SIR,

When the full moon is just rising above a clear horizon, she is an object that pleases every eye. But at the same time that she pleases the eye of the philosopher, she embarrasses his reasoning in attempting to assign the cause why her apparent magnitude is greater near the horizon than at higher elevations.

When men in former ages began to reason on this phænomenon, they imagined that the angle subtended by the moon was really increased, by a refraction of her rays, in passing through the vapours contained in the air near the earth's surface. But as soon as it became known that she subtended the least angle at the eye when her apparent magnitude was greatest, philosophers said that the eye was imposed upon by the long series of objects interposed between the eye and the extremity of the sensible horizon. And after it was discovered that the same phænomenon was observed at sea, where no land objects could be seen, they blamed the clouds for deceiving them; and when the clouds flew away, the spirit of inquiry flew after them, to seek for information in the apparent concavity of the sky.

Such were the erroneous opinions maintained by men of the greatest celebrity, both in antient and in modern times. To mention any more of them would be useless labour: a single quotation from Dr. Smith's Optics will, I presume, be quite sufficient to show how little was known of this matter at the time when he wrote.

After the professor had finished his explanation of this

* In Mr. Nicholson's Journal.

phænomenon,

phænomenon, he justly acknowledges that at different times the moon appears of very different magnitudes even in the same horizon, and occasionally of an extraordinary large size, of which he is not able to give a satisfactory explanation.—*Smith's Optics*, vol. i. p. 63, &c.; *Remarks*, p. 53.

It is really astonishing that this phænomenon should have remained for more than 2000 years without an explanation founded on a better principle than that of mere opinion.

That the dimensions of the pupil of the eye alter by the stimulus of light, is a truth well known; and it is also true, that the picture of the moon, formed upon the retina is not permanent, but varies as the dimensions of the pupil vary.

To demonstrate this property of the eye, let ACB, (fig. Z, Plate VII.) represent a plano-convex lens; PA and RB two pencils of white or compound rays of light, falling upon it at the points A and B, in a direction parallel to its axis: also, let Axv and Byv be the red or least refrangible rays, and Aqy and Bqx the violet or most refrangible. The red ray from A will cut the violet ray from B at the point x, and the red ray from B will cut the violet ray from A at the point y; through these intersections draw the line xy, and this line will be the diameter of the least circular space into which all the rays that fall upon the lens parallel to its axis can be collected; and this circle, which for brevity's sake is called the circle of aberration, is the true focus of the lens, or place where the image of the object is formed.

Let the sine of incidence going out of glass be n, the sines of refraction (into air) of the least and the most refrangible rays be p and q; then if a plano-convex lens be exposed with the plane side to the sun, the diameter of the circle of aberration xy (or image of the sun formed of rays of different refrangibility) is to the diameter of the lens AB as $q - p$ to $q + p - 2n$*.

From this given ratio of AB to xy it follows, that the image of the sun within a telescope varies with the aperture of the object glass.

Sir Isaac Newton found, by most accurate experiments,

* This theorem is well known to mathematicians.

that where the sine of incidence was 50, the sines of refraction of the red and the violet rays were 77 and 78. Hence $q - p$ is to $q + p - 2n$ as 1 to 55. And therefore AB : xy : : 55 : 1; or $xy = \frac{AB}{55}$.

To elucidate this theorem by examples, let the diameter of the object-glass of a telescope which is four inches be contracted to three inches, and afterwards to two; then the diameters of the circles of aberration formed by parallel rays will be $\frac{4}{55} = \cdot 072$, $\frac{3}{55} = \cdot 054$, and $\frac{2}{55} = \cdot 036$ respectively.

The same Property of Vision demonstrated otherwise.

Thus, let n represent the sine of incidence, and p and q the sines of refraction, as before.

The sine of incidence of every ray is to its sine of refraction in a given ratio *.

And the sine of incidence of the extreme ray PA varies with the aperture of the lens: for n becomes less as PA approaches the axis of the lens Ev.

Therefore the sines of refraction p and q, and the angle xAy, and its subtense xy, increase or decrease with AB: consequently, the image of the sun or moon upon the retina increases or decreases in magnitude with the pupil of the eye.

Now as the rays of artificial light are differently refrangible, it is evident from the given ratio of AB to xy, in which they increase or decrease at the same time, that the image of a candle formed in the focus of a convex lens decreases with the aperture of the glass. For the rays of the sun and the light of a candle are both governed by the same law in the formation of images in the focus of a lens; but this law does not obtain in the same degree in both objects, in consequence of the rays of the latter being in a more diverging state than those of the former.

Experiment I.

To prove the truth of this theory by experiment, I took two double convex lenses, each four inches in diameter and

* Newton's Optics, p. 64.

24 inches

24 inches focal distance, and fixed them in a frame close to each other in the same plane. This instrument I placed in a perpendicular direction, with a white screen behind it, to receive the images of a lighted candle that stood before it at some distance. The screen being moved until the two images of the candle were seen very distinctly upon it, they appeared exactly of the same magnitude; but when the aperture of one of the glasses was contracted to an inch in diameter, the image of the candle formed by that lens appeared three or four times less than the image formed by the other; and as these two images were seen at the same time, the eye could judge more accurately of their dimensions than of the apparent magnitude of the moon near the horizon and on the meridian. But when I moved the screen until the circular spot of light or spectrum formed by the lens of four inches aperture measured one inch in diameter, the spectrum formed by the other lens measured only one quarter of an inch; and as the screen was moved from the glasses by slow degrees, the spectrums decreased in the same ratio, until two perfect images of the candle were formed.

Experiment II.

In order to exhibit a more exact representation of the phænomenon, I had a frame made to contain seven convex lenses fixed in the form of a semicircle. They were all of the same focal distance, but not of the same aperture. The two largest apertures measured each 1·2 inches; these were to represent the pupil of the eye when viewing the moon at her rising or setting; the two next to these had each an aperture of 0·6 inches; the other two were still less; and the single lens at the top of the curve had the least aperture, to represent the pupil when the eye is exposed to the moon on the meridian.

This instrument being fixed in a perpendicular position, with a candle before it, as in the preceding experiment, and the screen brought to a certain distance within the focus of the glasses, the spectrums upon it appeared in the same ratio to each other as the glasses that formed them. For a representation of these spectrums see Plate VI.

Hence the cause of that variation which obtains in the apparent magnitude of an object, is no longer a matter of mere opinion; for it is an established law in dioptrics, that the image in the focus of a convex lens increases with the aperture of the glass, and consequently, the picture of the moon in the focus of the crystalline lens of the eye increases as the pupil is enlarged. I am, sir,

Your humble servant,

Ez. WALKER.

XLIV. *Extract of a Letter from M.* LEOPOLD DE BUCH, *of Milan, to Professor* PICTET, *of Geneva, on the Production of Muriatic Acid and Soda by the Galvanic Decomposition of Water* *.

SIR, Milan, October 6, 1805.

I HAVE a few words to say on the subject of a fact which at present occupies the attention of the Italian philosophers, and particularly Volta. It is the experiment whereby we compose the muriatic acid and soda by means of the Galvanic action. We have been told that this experiment, for which we are indebted to Dr. Pacchiani, has not succeeded at Geneva †. The following is the method to make it succeed at all times:

Have two piles, the one near the other, and joined together at the base, in such a manner, that the positive pole of the one and the negative pole of the other shall be at the superior extremities of the two piles. Conduct a gold or platina wire from the *negative* pole (of zinc) into a tube A, slightly closed at the top by a piece of linen or a cork, through which the wire passes in descending down about two-thirds of the tube. Make another wire of the same substance also descend from the positive pole (of copper) into a tube B, disposed in the same manner as the preceding. Plunge both of them into a drinking-glass containing distilled water. Oxygen will be disengaged in the tube B, and

* From *Bibliotheque Britannique*, vol. xxx.

† We do not believe it was ever tried at Geneva.—PICTET.

hydrogen

hydrogen in the tube A. Allow the apparatus to work, and a few hours will furnish unequivocal marks of the existence of the muriatic acid in the tube B, which had allowed oxygen to escape, and of soda in the tube A, which retained it. At the end of ten or twelve hours, nitrate of silver, poured into B drop by drop, precipitates itself abundantly. If the water of the two tubes is mixed together and evaporated, a sensible quantity of muriate of soda is obtained. If the experiment is made in the dark, the acid obtained in the tube B is oxygenated. The citron yellow colour is very striking; the smell of the acid cannot be mistaken; light decomposes it. The tubes must not be too narrow; they ought to be six or eight lines in diameter: without this precaution the experiment will not succeed; because the current, in order to pass from B to A by the inferior orifice of the two tubes plunged into a bad conductor, which is water, requires a cylinder of a great enough diameter in order to carry, in a given time, a sufficient quantity of electricity into the tubes. The latter must not be too far from one another; they will be at a convenient distance in a flat-bottomed drinking-glass. The oxygenated acid soon attacks the gold. If evaporated, we ought to have a fine purple of Cassius* for a residue. Has any thing been discovered for a long time more perplexing or more remarkable? The discovery of soda belongs to Mascagny, of Sienna†. I have the honour to be, &c.

LEOPOLD DE BUCH.

* The author is here in an error. There can be no precipitate of Cassius formed without the presence of tin.—EDIT.

† The production of soda by the Galvanic action is a new fact; but that of an acid (then supposed to be the nitrous acid) belongs to Cruickshank: he announced it very learnedly in a memoir entitled "Additional Remarks on Galvanic Electricity," published in Nicholson's Journal for 1800. What M. de Buch has here said relative to certain conditions of manipulation, without which the experiment did not succeed, will perhaps explain the reason why Messrs. Biot and Thenard met with no success in their attempts to repeat. —PICTET.

The production of soda by the Galvanic decomposition of water, was announced in The Philosophical Magazine so far back as the month of April last; that is, exactly a year ago. M. de Buch's letter given above is dated in October; and the number of M. Pictet's Journal, from which we have copied it, is that for November last. The first discovery, therefore, belongs to Mr. Peel. See our xxist volume, p. 279.—EDIT.

XLV. *Twenty-seventh Communication from Dr.* THORNTON, *relative to Pneumatic Medicine.*

To Mr. Tilloch.

No. 1, Hinde-street, Manchester-square, April 2, 1806.

DEAR SIR,

THE following cases of catalepsy and epilepsy are of considerable importance, and merit a place in your philosophic journal.

Case of Catalepsy cured by Vital Air.

Miss *Lavinia de Yrujo*, æt. 10, lives at Mrs. Baylis's, Brook Green House, Hammersmith. She had a fit at three years old: then her next attack was when five years old: when eight, they would attack her two or three times a week. She used to faint; and her eyes became fixed, her fingers clenched, and all her limbs completely rigid; the eyes would remain wide open and fixed: this would continue a quarter of an hour. The last fit, before she applied to me, was in October 1804, when it came on at two and continued till seven. Mr. Flower was sent for, who judiciously employed the usual restoratives, and advised the young lady to be sent home for further advice. She began the inhalation of a gallon of vital air, diluted, once a day, with the usual tonic remedies; and has not had a fit now, September 16, 1805, a whole twelvemonth. She only took the vital air for three weeks, and has not had any medicine, or the least occasion for any, since. She is now before me, with her mother, perfectly well; and is returned to her school at Hammersmith. The apothecary to the family, and several other medical gentlemen, had pronounced, to the mother and family, this case as one that never would be cured by art.

Observations on this Case by Dr. Thornton.

1. These cases yield to the vital air when all other remedies fail. The cause is, that the *vital air* gives energy to the muscles, and thence to the nerves, taking off inordinate action from an undue balance of principles.

2. And hence it is, that persons breathing much *bad air* become, on the contrary, convulsed. I am, &c.

ROBERT JOHN THORNTON.

XLVI. *Twenty-*

XLVI. *Twenty-eighth Communication on Pneumatic Medicine, sent by Dr.* THORNTON, *from Mr.* HILL, *Surgeon.*

Case of Epilepsy cured by Vital Air.

CHARLES WAYTE DARE, son of Mr. Dare, Dowgate Hill, is a young man who appeared strong and healthy until eleven years of age. At that period, being very abruptly informed that his father's house was on fire, the shock affected him so much as to throw him into an epileptic fit. From this time the paroxysms failed not to return every three or four weeks; and though, during the course of some years, the number of his fits did not much increase, his bodily strength and mental faculties were considerably impaired. Several medical men were consulted on the first attack, but the remedies prescribed by them gave little or no relief. On his becoming deaf in one ear, and his eyesight failing him, whatever he learned at the Blue-coat School was forgotten, owing to the violence of the fits. His parents, therefore, thought it advisable to take him home. At length, having received some benefit from medical aid, his father placed him on trial at Mr. Davison's, in Sise-lane; but a fright soon bringing back all his former disorders, he was obliged to return again to his family. His fits were now exceedingly violent, and their frequency increased to a considerable number in twenty-four hours. In consequence of this he became more deaf than ever, his vision weaker, and his intellects so materially injured that it was impossible to leave him alone, for fear he should either fall into the fire, or meet with some other calamitous accident. In this truly deplorable state he was put under my care early in March 1796. The morning previous to my seeing him, his fits had been so particularly violent as to exhaust him greatly, and his pulse beat above one hundred in a minute. It was not without infinite difficulty that I could either persuade him, or make him comprehend in what manner, to inhale vital air from my apparatus; and the general torpor of his mind, extreme debility of body, and deafness, gave

me but faint hopes of his recovery. However, after he had inhaled a moderate dose of vital air, an unusual warmth diffused itself over his whole frame, accompanied with a considerable degree of perspiration. He afterwards passed the whole day, and the following night, without any return of fits; a circumstance which had not happened for several months. The next morning he was tolerably cheerful; his hearing and vision less defective; and his pulse more firm, beating ten strokes less in a minute than the preceding day. On having again recourse to the vital air, it revived him as before, and the second day passed without a fit: but he found a disposition to fulness in his head, and such a tendency to falling down during that day, that it would have taken place, had not his own exertions prevented it. The third morning, before he came to my house, he was attacked with a very slight fit. Finding in him this tendency to local fulness in the head, I ordered cupping, and an opening medicine. By paying due attention to the fulness in the head, and keeping the bowels properly open, the pulse became regular; while the active effects of the vital air so invigorated his constitution, that he not only lost his fits, but in six weeks gradually regained his vision and hearing, and was able to walk six or seven miles a day, without fatigue, or any inconvenience whatever. Some sultry weather coming on in the month of May, he became nervous; had the headach, and some slight degree of fever, after a fatiguing walk to Hampstead; and for the second time only experienced a trifling relapse. I now directed him to be bled with leeches on the temples, and to take the usual dose of opening medicine; after which, as soon as the fever had subsided, he was to have recourse to the bark and vital air, at different intervals, until the middle of July. He then became perfectly well in health, strength, and spirits; and in December 1797 his father engaged him as clerk to Messrs. Hopkins and Lincoln, in Barbican, where he now resides; and, not having had any return whatever of his former complaints, he is fully enabled to keep such accounts as require a mind perfectly free from every degree of oppression or irritation.

"*To Mr. Hill.*

"SIR,

"I can with pleasure inform you, that my son, Charles Wayte Dayre, has, by the blessing of God, with your kind attention to him, and the help of vital air, received a very great cure from his fits, deafness, and nervous complaints, which had long affected him; and they increased on him so fast, that, when he applied to you, he had from sixteen to twenty a day. He could not be left at any time, even a quarter of an hour in a day. He has not had a fit these eighteen months, or near two years.

"I am, sir,

"Your greatly obliged humble servant,

"CHARLES DARE."

Observations on this Case by Mr. Hill.

However the general appearance of this young man may have been as to strength, some peculiarity of habit, as irritability of stomach and bowels, most likely had existed, and was a predisposing cause of the complaint. Be this as it may, any sudden surprise or misfortune will almost always produce some determination of blood to the head, more or less violent, in the strongest frame. In this case, as in many others, it laid the foundation of very serious mischief. In length of time it exhausted the nervous energy; and the powers of life, depending on an equable circulation, were reduced to extreme debility. Under these circumstances no remedy, one short instance excepted, arrested the progress of the disease, still less gave hopes of a recovery. The success in this case was beyond my expectation; for I was apprehensive that the several organs of sense, as the eyes and ears, were become paralytic from some organic defect in the brain, owing to the long continuance, violence, and frequency of the attacks. Contrary to my conjecture, however, the patient was relieved much in the same manner as the subject of the preceding case; and, as the same consequences followed, nearly the same reasoning applies to both; viz.

viz. some accumulation in the system being removed by the chemical union of vital air in the blood, the secretions by the skin and kidneys being promoted, and the energy and strength of the nerves being restored, then tonic remedies recovered the chylopoietic viscera to their due functions.

I cannot avoid particularly observing, that this lessening of the determination of blood to the head, is a fact of great importance to all people subject to fits or nervous head-achs.

XLVII. *On the Electrogene of* Schmidt. *By Count* Sternberg, *Vice-president of the Electoral Regency of Ratisbon*.*

A great deal has been said, for a year past, of M. Schmidt, a chemist of Breslau, who insists that he has merited the prize of sixty thousand francs, which ought to have been decreed to the author of the best discovery upon Galvanism. The following is a succinct account of what has been published on the subject, extracted from a letter addressed to M. Nauche, the physician, at Paris, by count Sternberg.

M. Schmidt thinks he has discovered a body capable of generating electricity, which he calls *electrogene*. This body serves, with caloric, to combine the two constituent principles of the air—oxygen and azote. Its affinity with them is weakened by the presence of the solar rays. The latter augment, on the contrary, its affinity with water: this is the reason why, in spring, electrogene partly quits the air to unite with water, elevated by means of caloric, in the form of elastic vapours.

The degrees of intensity of the union of electrogene with air and water are very variable, and thence proceed the variations in the elasticity of the atmosphere.

The sea, by surcharging the atmosphere with aqueous vapours, absorbs electrogene, which endeavours to maintain an equilibrium. For a contrary reason, the earth tends to give the air a part of its electrogene. This alternate combination and

* From *Biblioth. Phys. Economique*, no. 4. an 14.

disengagement

disengagement produce, by their succession, winds and clouds.

All the metals contain electrogene, and they do not oxidate but in regard to their affinity with it. It is the cause of crystallization by the dry method.

The weakening of the affinity of electrogene with the bases of the air, by means of the solar rays, gives birth to vegetation. Animal life is also attributed to electrogene. It enters with oxyen into the blood of animals by respiration; it is decomposed by this fluid, and its elementary base constitutes, in the brain and the nervous system, the principle of irritability.

M. Schmidt attributes still other effects to this new being; but he does not distinguish them well enough from those of caloric, oxygen, and attraction. His theory, although curious, is not sufficiently supported by facts to entitle it to much attention.

XLVIII. *Letter of M.* Orsted, *Professor of Philosophy at Copenhagen, to Professor* Pictet *of Geneva, upon Sonorous Vibrations* *.

SIR, Copenhagen, May 26, 1785.

The impartial interest which you take in every thing tending to accelerate the progress of science, has made me desirous, for a long time past, of establishing a correspondence with you. I take the opportunity of a traveller going to Geneva, to deliver to you the results of some of my researches in physics. I have chosen as the subject of my present letter, the experiments I have repeatedly made, upon the effects produced in the interior of solid bodies during the propagation of motion. I have been led to these researches by both antient and modern observations upon sound. Every body is acquainted with the interesting dis-

* From *Bibliotheque Britannique*, vol. xxx.

covery

covery of the celebrated Chladni, who taught the art of producing certain figures, by covering with sand plates of metal or of glass, and then rubbing them upon the edge with a violin bow. The sand is put in motion by the effect of the oscillations of the sonorous body, and the grains of it, quitting certain parts of the surface, heap themselves up upon the others, by forming lines and figures of different forms. The lines into which the grains of sand heap themselves up, are without doubt the points of rest between the portions of the surface put in vibration by the rubbing of the bow upon the edges of the plates.

Modern philosophers think they have found, in these curious results, a confirmation of the opinion which tends to exclude those internal vibrations, formerly supposed to be the cause of the phænomena of sound, and to refer the latter merely to those oscillations which are visible and appreciable by their mechanical effects, of which the experiment upon sand gives an example.

It appears to me, however, that the antient hypothesis has been abandoned too easily, at least my experiments have rather led me towards that hypothesis, than removed me from it. The following are the observations which these experiments have suggested to me:

It is allowed that in common air, a fluid eminently elastic, a sudden compression is followed by a reaction which produces a dilatation, which immediately compresses the neighbouring particles, and the effects of a first concussion propagate themselves in this manner to an indefinite extent. But I do not see why this does not take place also in elastic solids, when they are made to produce a sound. The experiments of Chladni, although in other respects very curious, are not proper to prove the small vibrations which concur to form the undulations from which the figures in question result, and for the knowledge of which we are indebted to him. Sand, which is made use of by him, is too large grained to indicate the nature of the movement of the solid moleculæ which tremble under it; and what is still worse, these grains are elastic, so that they do not remain where they

they fall, but fly about; this hinders them from pursuing a regular progressive movement, like that of sonorous undulations. It was on this account that, on varying my experiments, I thought of substituting in the place of sand, a more attenuated matter not possessing the inconveniences I have mentioned; this was *semen lycopodii*, or the seeds of club-moss. After having covered a plate of metal or glass with this substance, I tried to produce a sound in the manner of Chladni, and in an instant I saw the dust distribute itself into a number of little regular *tumuli*, which put themselves in motion at their extremities, or formed the figures discovered by this naturalist. They always range themselves in the form of a curve, the convexity of which is in proportion to the point touched by the violin bow, or towards the point which has an analogous situation: the nearer that each of these little heaps is to these points, its height is the more considerable; this gives to the whole a remarkable regularity. The experiments of M. Chladni are astonishing at first sight, on account of the regularity of the figures produced by a single stroke of the bow, which appears as if done by magic. My experiments have not the same charm; but perhaps they are more instructive, on account of the comparative slowness of my process, which admits of their effects being more advantageously studied. We observe that the interior of the small elevations formed in my experiments continue in motion during the continuance of the sound. The duration of these vibrations, although very short, is appreciable on plates of three or four inches diameter: it is longer when plates of a larger diameter are employed. I often made use of a disc of metal of six inches diameter: in this case I always saw the small elevations change their appearance at different epochs of the duration of the sound. At one moment the height increases, and at another it diminishes, and the dust has the appearance of arranging itself in small globules which roll one above another: we easily perceive that all these phænomena are still very complicated. The movement of the grains is in part vertical, in part horizontal; the horizontal movement is composed of two others; one of the forces impels the grains forward, and

the other drives them to the two sides. I was anxious to examine, as minutely as possible, each of these movements by itself, and the following is what I remarked. I held a square of glass in such a manner that two or three of the edges of it were in contact with my fingers. I struck the edge which I did not touch, with a piece of smooth wood, in such a manner that every point received the shock at the same instant. In this case, the powder arranges itself in lines parallel to the edge struck upon. These lines are rarely straight, because these edges have always irregularities; the straightest I ever saw were produced upon an iron rule, struck with a smooth piece of the same metal: in order to the better success of this experiment, it is necessary that the square or the rule should rest upon a smooth table. If I do not strike the blow upon the whole edge, but only upon some of its points, other lines are formed parallel to the direction of the blow, and perpendicular to the edge struck. These lines seem to be composed of a crowd of small elevations, less regular than those produced when plates covered with the dust are struck with the fiddle bow. This experiment succeeds with difficulty when the whole of the plate is allowed to rest upon a table, because this circumstance prevents us from striking a good blow. In short, if I struck the plate in a direction perpendicular to its plane, the dust arranged itself in little regular heaps. In the former case the undulations proceed forward; in the second case, they take place both in a forward and a lateral direction; in the third case, they proceed in every direction, because the compression produced by the blow perpendicular to the plane, propagates itself as well horizontally as vertically. The tone produced in the first case is very low; that of the second is much more elevated; but that of the third is still much more elevated; and it is the only one which deserves to be called a tone, or a sound appreciable in the musical scale.

One would suppose that the change produced in elastic bodies, by the communication of motion, could scarcely be limited to the simple mechanical displacement of the part, but that in this modification it ought to have some other more

more intimate action. Every kind of friction produces not only heat, but electricity also. De la Place, and Biot, have already attracted the attention of philosophers to the first of these phænomena; I am of opinion that the latter of them requires much more attention. I always found in my experiments that sand, or dust, adheres much more to those parts to which the movement of the sonorous bodies had fixed it, than it did to the other parts. I have often thrown fresh sand over a plate of glass, upon which I had already produced a figure. I shook it gently after having reversed it, and I always remarked that the sand which formed the figure remained adhering, while the other part detached itself. The adherence of grains finer than those of sand is very remarkable. I also discovered, with the assistance of Coulomb's electrometer, indications of electricity in those plates which had emitted a sound; but I have not repeated these experiments sufficiently to enable me to detail them. I discovered on the above occasion, that the edges and angles of bodies act upon Coulomb's electrometer almost always; and I propose to myself a new course of experiments upon this subject. The celebrated Ritter, to whom I had communicated my experiments upon the part which electricity acts in the phænomena of sound, had long ago discovered that the electrical pile of Volta is capable of producing sound, when a shock is received from it in the ears. In a work about to appear under the title of "A System of Electrical Bodies," this great philosopher makes it clear that a body acquires positive electricity by compression, and negative by dilatation. Thus we may say, that there are in each sound as many alternatives of electricity, positive and negative, as there are oscillations; but the union of two electricities produces a commotion: thus there are in one sound as many extremely weak electrical commotions as there are oscillations. Each of these insulated commotions would be absolutely insensible; but when received in a very great number, in a period too small to distinguish the one from the other, they always produce a sensible effect, especially since positive electricity renders the organ more sensible for the negative than it was before, and *vice versâ*.

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The sensible effect of the union of all these insensible commotions is sound. I confess that these ideas of M. Ritter appear contradictory to all the received opinions on the organ of hearing; but it must also be confessed that our knowledge of all the organs of sense is as yet imperfect. I am of opinion, however, that the theory of M. Ritter agrees perfectly well with the antient hypotheses. As for my own experiments, they may be easily repeated by any person, and some one perhaps may discover more than I have here described.

XLIX. *On the Use of the Sutures in the Skulls of Animals. By Mr.* B. Gibson*.

The full use of the singular junction of the bones of the skull which is called suture, has, from the earliest periods of anatomy and surgery, attracted the attention and eluded the researches of the physiologist. To this remarkable feature in osteogeny, in a great measure peculiar to a certain period of life, many uses have been attributed. Some of these are totally erroneous; such as that for allowing the transpiration of moisture, to keep the brain cool and fit for thinking; for giving a more strict adhesion of the *dura mater* to the inner surface of the skull; for admitting a more free communication by blood-vessels between the external and internal parts of the head; or for affording interstices, that the bones may be pushed asunder by the growth of the brain, lest that organ should be cramped in its growth, in consequence of the comparatively slow growth of the bones of the skull.

Other uses attributed to the sutures are merely slight advantages derived from their structure, which are enjoyed in early infancy, or till adult life, but gradually cease after that period. Thus at the time of birth the loose union of the bones of the skull accommodates the shape of the head to the figure of the different parts of the cavity through which it passes. At adult age, when the sutures are fully formed,

* From *Manchester Transactions*, second series, vol. i.

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they may occasionally check the progress (if I may be allowed the expression) of a fracture nearly spent;—or vibrations, communicated to the bones of the skull, will be propagated with less force to the brain, in consequence of the bones being separated at the sutures. It is, however, abundantly evident, that these are not the main purposes for which the sutures are formed; otherwise they would not begin to be obliterated at a period of life when they would perform these offices more usefully than ever. Consistent with this remark, we shall find that the true purpose for which they are formed, and the particular process with which they are connected, is fully completed before their obliteration takes place.

When we take a view of the mode of junction between many bones and parts of bones in the human body, which do not admit of motion, we find that with little exception they all agree in this particular; that sooner or later the cartilage or periosteum, which once was interposed, is obliterated, and these different portions, or entire bones, coalesce.

The separate portions, which originally compose the vertebræ, are early in thus uniting: after these the sides of the lower jaw; at a later period the epiphysis of a cylindrical bone is united to its body: and still later the bones of the skull usually coalesce, and the sutures are obliterated. Other bones, as those of the face, which have no motion and sustain little weight, are irregular in this respect; sometimes uniting, but generally remaining distinct to the end of a long life.

The original formation of the osseous system in several distinct pieces, respects principally its speedy ossification at an early period of life, and its future convenient extension, till it has arrived at its full growth; and we may consider it as a general principle, that where two parts of *one* bone are separated from each other by an intervening cartilage, or two distinct bones merely by periosteum, at that part osseous materials are added to increase their length or extend their superficies. This we shall find takes place, whether the junction be effected by comparatively smooth surfaces, as between the body of a bone and its epiphysis; or between

the bones of the skull by jagged sutures. Hence it appears that the bones of the body generally are increased in length or extent, not by a uniform extension of the whole substance, but by an addition of bony matter in some particular part.

Thus the body of a cylindrical bone is lengthened by addition to each end. This we might conclude would be the case, from considering the part in which its ossification commences: as this commences in a middle point and proceeds to each extremity, it is natural to suppose that its growth still goes on in the same direction, or continues at the extremities. That this is the case we know, not by reasoning alone, but by a direct experiment. Mr. Hunter sunk two small pieces of lead in the middle of the tibia or shin bone of a pig, and measured accurately the distance between them: on examining the animal some time afterwards, it appeared, that though the bone had increased considerably in length, the pieces of lead still remained at the same distance from each other that they were before. From this experiment we learn, that a cylindrical bone is not extended in its middle, but is lengthened by addition to its extremities, where the body of the bone is joined to its epiphysis; the chief intention of the epiphysis being to allow the intervention of a vascular organ, which may conveniently deposit bony materials, without interfering with the joint itself.

As cylindrical bones are lengthened at their extreme parts, we are led by analogy to conclude, that the same general plan is pursued in the extension of the flat bones of the body: and although we have no direct experiment by which this has been proved, there are circumstances which leave little doubt but they are extended by addition to their edges. Thus, to take the parietal bone as an example; as ossification begins in a central point and extends towards the circumference, it is probable that, to the completion of the process, it continues to go on in the same direction; and the same circumstance taking place in every bone of the cranium, it is probable, that even after the whole of the brain is incased

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in bone, the addition is still made at the edge of each, and that the general enlargement originates where they are all mutually joined by the sutures. Of this process I had a very striking illustration some years ago. In a young subject, from what cause I know not, the deposition of osseous matter had been suddenly increased a short time before death. It was in different stages of progress, but had taken place in all the bones of the body which I preserved; in some partially, in others generally. In all, the new osseous matter was elevated above the level of the bone, upon which it was placed. In some parts of the parietal bones it was only in its commencement, and put on the appearance of a net-work, similar to that which may be observed in the same bones at an early period of their formation. In other parts the meshes of the net-work were more or less filled up; in others again completely, so as to put on the uniform appearance of solid bone. The same reticulated appearance was evident on the edges of all the bones of the skull, where they form the sutures, and at the extremities of the cylindrical bones, between the body and epiphysis. The same appearance of increased deposition was seen on the surface of the cylindrical bones, with this difference, that the meshes were not circular, but oblong squares; so as to put on more of the striated appearance. In some parts, the newly secreted bone was easily separable from the general mass, and formed a thin layer externally, affording one of the best proofs I have met with, of the increase of cylindrical bones in thickness by deposition externally, whilst a corresponding internal absorption goes on. From the striking similarity of appearance on the surfaces and edges of the bones, we may safely conclude, that the same process of deposition was going on in both, and may thence infer, that the bones of the skull are increased in extent by the deposition of osseous matter at their edges, or where they are joined to each other by suture. This fact points out to us, in a great measure, the real use of this peculiar mode of junction.

In order that the bones of the skull may be increased in extent, it is necessary that they should be retained at a certain distance from each other; that the periosteum with its

vessels may pass down between them, free from compression, and secrete the osseous matter. At the same time, the thin bones composing the upper part of the skull, resting as an arch upon its basis, must be united together so firmly, as not to be separated by common degrees of violence. For this purpose, projecting points from the external surface of each bone are reciprocally received into corresponding niches; which only penetrate through one half of the thickness of the skull, and form an irregular kind of dovetailing.

Two advantages arise from this structure being superficial, and confined to the external table of the skull. The projecting points from each side, resting upon the solid surface of the internal table of the opposite bone, can resist more effectually any violence which might tend to force the bones inwards; and the internal part of the skull presents, by this means, a smooth surface to the coverings of the brain; for internally no appearance of a jagged suture is seen.

From this view of the subject we see, that the sutures of the human skull, by their peculiar formation, at once unite the bones together, and so far separate them, as to allow the interposition of a vascular organ by which their superficies is gradually increased to its greatest extent.* This explanation

* Since this paper was written in the year 1800, I have found, that a similar opinion was published by Professor Soemmerring in 1794, in his valuable work, "De corporis humani fabrica." To him, therefore, any credit which may belong to the primary suggestion of this use of the sutures is due. As his opinion, however, has been little noticed by anatomists generally, and is placed in a clearer point of view by the facts which suggested this further explanation of it to me, it has not been thought improper to give this essay a place in these Memoirs. But whilst the reader will see, by the following quotation, the near resemblance between the opinion of Professor Soemmerring and that which I have brought forward, I hope the character of plagiarist or compiler will not be attributed to me.

"Usus horum sic sese habentium terminorum ossa cranii inter bene liquet.

"Incrementum ambitus calvariæ levant, ni enim inter ossa capitis mox post partum suturæ interponerentur, hæc crescere non possent, nisi aliâ ratione natura rem institueret. Tali igitur modo incrementum calvariæ cum incremento reliquorum ossium convenit; initio enim suturis, vel potius lineis cartilaginosis ossa iis locis conglutinantur, verum tamen non nisi in embrionibus ad

tion of the use of sutures comprehends and accounts for those concomitant circumstances, which were considered by older anatomists as their real use; and, as far as I can see, is not contradicted by any fact connected with them.

If it be asked, for instance, why at the sutures there is a stronger adhesion of the dura mater internally, and periosteum externally, than in other parts of the skull? the answer is, that these membranes with their vessels are continued into the sutures, to form conjointly the secretory organ by which the bones are extended.

If it be asked, why there is a greater vascularity or an appearance of blood-vessels passing through the sutures? it is perfectly consistent with this opinion to answer, that the increase of blood goes to this secretory organ, for the purpose of the extension of the bones.

The explanation here offered accounts also for the general obliteration of the sutures after a certain period of life; for, the bones having then arrived at their full size, the organ for the secretion of osseous matter is no longer needed; it shrinks and is absorbed, and the bones gradually coalesce; by which a further advantage is derived, that of an accession of strength to the cranium at large.

If any additional argument be necessary in support of this opinion, I may also notice the striking analogy which subsists between the separation of one bone of the skull from another by a suture, and that separation which exists be-

ad fonticulos, ut aiunt, hæc linea notabili latitudine, observatur. Ossibus enim capitis hic locorum cerebro crescente, placide quasi diductis, cartilago augetur, latior evasura, nisi pristina pars simul in os mutaretur, inde ossa calvariæ, eodem modo, quo ossa longa diductis epiphysibus, vel quod unum idemque est, marginibus crescere, liquet, etsi in ossibus, longis sutura epiphyses inter et diaphysin non crispetur.

"Quo junior igitur infans, eo minus crispa et implexa sutura, vel, ut rectius loquar, linea cartilaginosa angusta, ossa jungens, observatur. Quum vero aucta ætate ossa, crescente cerebro, diducuntur, eorumque crassitudo, adposita cum internæ tum externæ potissimum tabulæ, (internæ enim incrementum citius absolutum videtur) massa ossea, augetur, non potest non esse, quin hæc crispa suturæ forma, quum quidem nasci cœpit, externâ in superficie tamdiu, augeatur, donec tandem ipsa ea quam maxime impediat, quo minus cerebrum calvariam ulterius diducere possit, quod pubertatis tempore accidit. Rarissime hæc ossificatio ad ætatem virilem usque detinetur."—Soemmerring De corporis humani fabrica, pag. 212.

tween the body of a cylindrical bone and its epiphysis. They each remain only for a certain length of time; each allows the interposition of a secretory organ; and both begin to be obliterated, when the bones with which they are connected have completed their growth, and their continuance is no longer necessary.

L. *On the Existence of Phosphate of Magnesia in Bones.* By M. Fourcroy *.

The discovery of a new earthy phosphate in the bones of animals, is a fact in the history of the physical sciences important enough to deserve a place in every journal calculated to promote improvements in natural history. This discovery has been made these two years by M. Vauquelin, and myself, in the course of a very extensive inquiry undertaken upon animal concretions, and of which we have from time to time given an account to the public.

Having discovered, 1st, the ammoniacal magnesian phosphate among the materials of the calculi in the human bladder, and among the intestine bezoars of animals; and 2dly, the phosphate of magnesia in human urine, and the property which characterizes it, in this liquid, of passing to the state of a triple salt very crystallizable, at the moment that the urine becomes ammoniacal by its spontaneous decomposition; it appeared to us important to inquire if this magnesian salt, unknown in animal matters until our inquiries, was not to be found in some animal organs; and we began our inquiries upon bones, the analogy of which with the urinary receptacles is so well known by physiologists. The analysis of these organs seemed to be brought to perfection; and yet the new experiments to which we submitted the substance under examination, enabled us to discover this salt by methods and processes much more complicated than those which have been hitherto employed in this species of analysis.

* From *Annales du Muséum d'Histoire Naturelle*, vol. vi.

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In order to procure the magnesian phosphate in an osseous base, the bones of oxen, after being calcined white and well pulverized, must be treated with an equal quantity of sulphuric acid; after being allowed to rest five or six days the mixture is diluted, at first with ten times and afterwards with five times its weight of water. The liquor is drained every time; ammonia in excess is poured into all the liquors mixed together; and as these liquors, which were formerly supposed to be the phosphoric acid, contain phosphate of lime and phosphate of magnesia dissolved in the phosphoric acid, in proportion as the ammonia saturates the acid, a mixture of calcareous phosphate and ammoniacal magnesian phosphate is precipitated. This precipitate when washed with a little cold distilled water is treated with a ley of pure potash, by boiling them together until no more ammonia is disengaged. The ammoniacal magnesian phosphate is decomposed by this process. The ammonia volatilizes, the phosphoric acid unites itself to the potash, and the salt resulting from it remains in solution; the free magnesia deposits itself in powder, and mixes with the phosphate of lime at the bottom of the liquor. The latter is now drawn off clear, the precipitate is washed, and boiling distilled vinegar is poured upon it, which dissolves the magnesia without touching the calcareous phosphate; the magnesian acetate is decomposed by carbonate of soda, and we have the carbonate of magnesia, which is dried, of which the weight is determined, and which is dissolved in the sulphuric acid in order to obtain it in the state of sulphate of magnesia, the form under which it is more generally and more certainly recognised.

By repeating this process a great number of times, in the first place to ascertain the discovery of the phosphate of magnesia in the bones of animals, and secondly to appreciate the proportion of it, we ascertained that the acetic acid always dissolves a little lime with the magnesia, and that this lime could not proceed from any thing else than the decomposition of a small portion of the calcareous phosphate by means of potash: thus, in spite of the laws of affinities hitherto determined, it appeared evident that potash sepa-

rates the lime of the phosphoric acid; but it only separates a very small quantity from it, and only when potash is employed by itself in a great quantity, while lime takes the potash from the phosphoric acid entirely and completely. We have here the effect of the power of masses, called in by Berthollet in order to explain some apparent anomalies in the action of the elective attractions.

Ox bones calcined contain about a 40th part of phosphate of magnesia.

Those of horses and sheep yielded a 36th part.

Those of the feathered creation, about a 40th part.

We were not able to extract, from any of the above, a quantity any thing equal to that contained in human bones.

We have to remark on this occasion, that ox bones, which we had most frequently and most exactly analysed, appeared to contain the following materials:

Solid gelatine	51·0
Phosphate of lime	37·7
Carbonate of lime	10·0
Phosphate of magnesia	1·3

The phosphate of magnesia existing in animals manifestly arises from their food. Not only have we found this salt in wheat, barley, oats, and vetches, but we discovered it in all the grasses named by Linnæus *cerealia*, in the proportion of a centieme and a half of their weight; which is almost double the quantity of the phosphate of lime which they contain. Without doubt, some relation subsists between this fact and those concretions found in the intestines of animals, which are formed of ammoniacal magnesian phosphate.

It is not impossible to conceive why human bones do not contain phosphate of magnesia, although this salt exists more abundantly than the phosphate of lime in flour, &c. It appears that it is evacuated by those urines in which we have discovered it, while it does not exist in the urine of such animals as throw it off by the skin or the intestines.

Without pushing too far the influence of chemical discoveries upon the animal œconomy, made by the analysis of

various substances, we cannot pass over, in each of these discoveries, immediate applications, which ought no longer to be neglected. Physiology must necessarily derive great advantages from attending properly to such results.

LI. *Notices respecting New Books.*

A Treatise of Mechanics, Theoretical, Practical, and Descriptive. By Olinthus Gregory, *of the Royal Military Academy, Woolwich. 2 Vols. 8vo, and a Volume of Plates. Published by Kearsley.*

It is with much satisfaction we announce the publication of a work of this nature, which has been long wanted.

The first volume is devoted chiefly to theory; the second is practical and descriptive. The theoretical part is divided into five books, and these are subdivided into chapters, as the nature of the several subjects seemed to require.

Book I. is appropriated to the subject of statics, and is subdivided into six chapters. It commences, as indeed do all the books, with definitions and preliminary remarks; which are succeeded by a statement and illustration of the Newtonian laws of motion and rest, which are assumed as fundamental principles to guide the subsequent inquiries. The nature of statical equilibrium, with the composition and resolution of forces, are then discussed; forces being considered, 1. as disposed in one plane, and concurring in the same point: 2. as directed to one point, but not confined to one plane: 3. as situated in one plane, but applied to different points of a body: 4. as not confined to one plane, and directed to various points of a body. The remaining chapters in this book treat of the centre of gravity, and the centrobaryc method; the simple machines, or, as they are usually called, the mechanical powers; the strength and stress of timber and other materials; and the equilibrium, tension, and pressure of cords, arches, and domes. The minnter topics connected with these general points of inquiry cannot well be specified here.

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The second book relates to dynamics. It comprehends six chapters; treating of motion, uniform and variable; the descent and ascent of heavy bodies in vertical lines; the motion of projectiles, with observations on ricochet-firing; descents along inclined planes and curves; the vibrations of pendulums, and the curve of swiftest descent; central forces; the rotation of bodies about fixed axes, and in free space, with theorems relative to the centres of oscillation, gyration, percussion, spontaneous rotation, &c.; the physico-mathematical theory of percussion, nearly as first delivered in Don George Juan's Examen Maritimo; and the motion of machines, and their maximum effects; closing with some remarks showing in what points of view machines ought to be considered by those who would labour beneficially for their improvement.

Book III. is devoted to the subject of hydrostatics. It is divided into four chapters,—on the pressure of non-elastic fluids; the determination of the specific gravities of solid and fluid bodies; the construction of hydrometers or areometers; the equilibrium, stability, and oscillations of floating bodies, with a particular reference to ships and canal boats; and on the phænomena of attraction in capillary tubes. This book contains an extensive and correct table of specific gravities.

In the fourth book are given some of the most useful propositions and theorems relative to hydrodynamics. It comprises four chapters. 1. On the discharge of fluids through apertures in the bottom and sides of vessels, and on spouting fluids: 2. An account of experiments made by different philosophers (as Bossut, Venturi, Eytelwein, Young, Vince, &c.) on the discharge of water through apertures and tubes; and the practical deductions from those experiments: 3. On the effect of water upon the motion of water-wheels: 4. An account of Mr. Smeaton's experiments on water-wheels.

The fifth book, which terminates the first volume, is on pneumatics, and contains six chapters. The subjects stated and discussed in this book are, the equilibrium of elastic fluids; the measurement of altitudes with the barometer and thermometer; the motion of air when the equilibrium of pressure

pressure is removed; the theory of air-pumps, and pumps for raising water; the resistance of fluids to bodies moving in them; and results of experiments on the resistance of fluids.

The second volume of this work, which, as before mentioned, is chiefly practical, commences with an introduction occupying about eighty pages, and comprising general remarks, rules, and directions,—on the construction and simplification of machinery; on rotatory, rectilinear, and reciprocating motions; on bevel-geer, and proportioning the number of teeth; on friction and the rigidity of cords, with the experiments of Vince, Coulomb, &c., and an example of the power of the capstan, allowing for friction and the stiffness of cords; on water and wind as movers of machinery, with Smeaton's rules relative to windmills; on the strength of fired gunpowder; on steam as a mover of machinery, with the theorems and results of Bettancourt and Dalton; and on the strength of men and horses, according to the best and most accurate observations. The remaining part of this volume is appropriated to the description of a variety of mechanical contrivances, in number exceeding an hundred and fifty. These are arranged alphabetically, that they may be consulted with most convenience; and several of them relate to machines and instruments which have never before been publicly described.

In the exposition of the theory the author has generally proceeded by a series of connected propositions and dependent corollaries; those in each chapter having a manifest relation to each other, and flowing naturally from the same source. He has avoided the two extremes,—a fatiguing prolixity of detail, which leaves nothing to be filled up by the ingenuity of the pupil, and that obscurity which often results from the suppression of intermediate ideas. Mr. Gregory has not attempted to explain the *nature* of gravity, impulse, or the other sources of the motion of bodies; and the reasons he assigns in his preface are perfectly satisfactory. The general definition of the term *force*, in a mechanical sense, supersedes the necessity of inquiring into the essence of the various kinds of forces which may operate upon

upon matter. Those who carefully contemplate the process of that gradual refinement of language, which results from the necessary demands occasioned by the progress of civilisation, will see how requisite it is to appropriate terms originally of a laxer or of a grosser signification, to some peculiar modification of thought; and hence, that such words as *power* and *force*, primarily used to denote animal energy, are now, by a natural extension, grounded upon an obvious analogy, employed to express *efficiency* in general. In the philosophical acceptation, then, he defines force or power to be that, whatever it be, "which causes a change in the state of a body, whether that state be rest or motion: and this definition does not require entering into any metaphysical disquisitions relative to the nature of causes, or the connection of cause and effect: that every effect is brought about by some cause, or something which precedes in the order of occurrence, is a truth which none will be disposed to deny; but what is the agency, or where it actually resides, we can seldom know, except, perhaps, in the case of our own voluntary actions. It is not, then, the business of the mechanist, strictly speaking, to inquire into the *modus operandi*: we learn, from universal experience, that the muscular energy of animals, the operation of gravity, electricity, pressure, impact, &c. are sources of motion, or of modifications of motion; and hence, without pretending to know the essence of either of these, we do not hesitate to call them mechanical forces; because it is incontrovertible that bodies exposed to the free action of either, are put into motion, or have the state of their motion changed. Forces, therefore, being known to us only by their effects, can only be measured by the effects they produce in like circumstances, whether those effects be creating, accelerating, retarding, deflecting, or preventing motions: and it is by comparing these effects, or by referring them to some common measure of ready appreciation, not by ascertaining the essential nature of any forces, that mechanics is made one of the mathematical sciences.

"Besides, what is meant by the *nature* of any thing? As we are ignorant of its essence, or what makes it that thing and no other thing, we must content ourselves with the discovery

covery of its qualities or properties; and it is the assemblage of these which is commonly called its nature: yet this is very inaccurate, since these are only the consequences of the essence. Hence we can give no definition of even the simplest of things which comprehends its real essence."

We have no hesitation in saying that this work cannot fail to prove extremely useful. The first volume, in particular, is executed in a manner highly creditable to the judgment and abilities of the author, who has rendered an essential service to his country by its publication. In praising in this particular manner the merits of the first, we would not be understood as meaning to censure the second volume: from the very nature of its contents it was impossible to infuse into it the mind which pervades the other; but the author has collected in it much useful information, which with less industry and judgment than he possesses could not have been brought together in the same compass. A future edition would probably be rendered still more valuable, by the suppression or alteration of a seemingly *ex parte* statement of the comparative merits of Mr. Watt's steam-engine and that of Mr. Hornblower; which however is not given in the work as the author's own production, but as from Mr. Hornblower, or some relative of the same name. Should the work meet with that reception which it merits from the public, we have no doubt but a new edition will soon be demanded.

A Treatise on the Teeth of Wheels, Pinions, &c., demonstrating the best Forms which can be given to them for the various Purposes of Machinery, such as Mill-work, Clock-work, &c.; and the Art of finding their Numbers. Translated from the French of M. CAMUS; *with Additions. Illustrated with fifteen Plates. Published by J. Taylor, Holborn.*

This work is a translation of the tenth and eleventh books of M. Camus's *Cours de Mathematique*. The abilities of the author are so well known to the learned world, that nothing that can be said here can add to his fame; and it is but justice to the translator to say, he has executed his part

part with fidelity, and in a way creditable to himself. It is of much more importance than mere operative mechanics can possibly conceive, that the best form of the teeth of wheels should be ascertained on true mathematical principles. Where this is not attended to, there will always be an unnecessary load on the machinery, arising from mere friction, a consequent waste of power and time, and a proportionate expenditure for repairs. We cannot however avoid noticing that there is a manifest disagreement between the text of this work, and an illustration of the application of the epicycloid to the teeth of wheels given in the preface. The latter directs the epicycloid curve to be generated from the primitive diameters of the wheels that are to work into each other; the former directs them to be generated from circles equal to radius only of these diameters.—This work, we doubt not, will be found very useful, and we hope will procure to the publisher a proper return for the service he has rendered to the public by presenting it in an English dress.

The Vaccine Contest; or "Mild Humanity, Reason, Religion, and Truth, against fierce unfeeling Ferocity, overbearing Influence, mortified Pride, false Faith, and Desperation," &c. By WILLIAM BLAIR, *M. A. Surgeon of the Lock Hospital, &c. &c. Published by Murray.*

In this work, designed for the use of clergymen, heads of families, and other unprofessional readers, Mr. Blair attempts to answer the objectors against the cow-pox, on quite a new plan; viz. by quoting their own words, and making them refute themselves; in which it is but justice to say, he has effectually succeeded. This popular work is a very fair exposure of the unprincipled means to which the anti-vaccinators have resorted to turn the prejudices of the ignorant into a source of dishonest emolument to themselves.

Doctor Patterson, of Londonderry, strongly impressed with the importance of the subject, is engaged in "Researches concerning Pestilential and Epidemic Diseases,

with a View to obtain valid Principles whereon to found a Civil Constitution of Medical Police for Ireland."

To every one conversant in this field of inquiry, it is manifest, that the climate of the country, the physical and moral attributes of the inhabitants, their political and commercial relations, their œconomical institutes, and other corresponding circumstances, must be taken into consideration in a work of this description. It will also be necessary to take a retrospective survey of the numerous malignant and reigning distempers which have prevailed in many parts of the earth throughout a series of ages. These are not easy tasks—they are not small undertakings: yet, difficult and extensive as they are, little short of their achievement will answer the purpose; a purpose which has in contemplation objects of the first national importance.

Mr. Rudge has just published the Fourth Fasciculus, which completes the First Volume, of his Descriptions and Figures of the Rare Plants of Guiana.

The plants described in this work formed a part of that superb collection of natural history consigned from Cayenne to the National Museum at Paris: they were five years collecting in Guiana, by order of the French government, and were captured on their passage by two British privateers at the commencement of the present war.

We understand that only 150 copies of this work will be published.

LII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

March 27. The Right Honourable President in the chair. —The reading of an interesting paper, consisting of observations on the marine barometer and thermometer, made by captain Flinders, on the coast of New Holland, in the years 1801, 1802, and 1803, commenced, and the society adjourned, in consequence of the Easter holidays, till the 17th of April. The President in the chair.—Continuation of

of the above paper, containing a great variety of isolated facts of the most extraordinary and unaccountable fluctuations or stagnations of the mercury in the barometer and thermometer.

On that evening Messrs. Lacépède and Cuvier, of Paris; M. Prevot, of Geneva; and Mr. Charles Harding, of Bremen, the discoverer of the new planet Juno, were elected fellows of the Royal Society of London, in the foreign list.

April 24. The President in the chair.—Continuation of captain Flinders's meteorological reflections, in which the author seemed to believe that he had accumulated sufficient data to enable him, with a seaman's experience, to prognosticate truly the general state of the weather, and the approach or continuance of a storm, by the assistance of his barometric observations. This desideratum is doubtless within the sphere of human science, and might be attained by attention and industry, only in the course of a few years, if every captain of a vessel would bestow on it the same attention as captain Flinders, and communicate his observations with equal promptness. The number, talents, and opportunities of our naval officers render it extraordinary that they have not before this time collected such a multitude of facts as the philosopher in his cabinet could arrange and digest into a practical system, that would enable the navigator to anticipate the direction and strength of the wind in every latitude, and at all seasons of the year. There exists, indeed, at the present moment, a great deal of meteorological knowledge, among individuals of our seamen, unknown to many of our meteorologists, and equally unprofitable to the public, merely from diffidence in the observers to write down their observations.

It may not be improper to add here a peculiarity of the marine atmosphere, not observed by this minute narrator of facts, nor perhaps by any other seaman, namely, that the air or wind always traverses the sea in distinct masses or bodies, and in parallel lines. It is a fact, to which there are few exceptions, that a current of air, whatever may be its direction or velocity, uniformly travels in a right line at sea, till it is either entirely absorbed or its power exhausted by

by rarefaction. The reverse is the case on land; as no current of wind can pass many miles without experiencing a change of temperature, density, velocity, and even direction, in consequence of the variegated surface and temperature of the earth. The rencounter of two opposite currents of wind, at sea, does not produce the effect proper to solid bodies in motion, according to the received laws of mechanics; on the contrary, as their temperatures and densities must necessarily be different, each pursues its undeviating course without any change, excepting only that the least dense is obliged to ascend and make room for the other, which keeps next to the surface of the water; notwithstanding which, each proceeds with nearly its original velocity. Two currents of air, whatever may be their temperatures or densities, never incorporate either by sea or land, without the intervention of a third body (such as mountains, &c.), which destroys their respective velocities. No such resisting bodies being at sea, the different currents of wind must consequently follow the law of their densities. Parallel currents often occur in very short distances, and the intervening spaces frequently experience dead calms, sometimes accompanied with fogs, or with unclouded brightness, and considerable heat. This circumstance will, on many occasions, account either for the stagnancy or rapid fluctuation of the mercury in the barometer at sea; which can never happen, even in the warmest or most electrical climates, on shore, where there is a much greater and more general sameness of density and temperature in the atmospherical currents, in consequence of the greater facilities of commixtion, and often, indeed, of chemical union of the different gases.

For these remarks, which form no part of captain Flinders's communication, we are indebted to a gentleman who has made a number of observations on this important subject. Should any of our nautical readers, or others who may have had an opportunity of making similar observations, communicate to us the result of their experience*, we shall gladly lend our aid to the collecting of such facts

* Addressed to the Editor of The Philosophical Magazine.

as may tend to the elucidation of this subject, by giving them a place in our work.

A letter from Smithson Tennant, esq. to the President, was read, announcing his discovery of native minium in a vein of galena in Devonshire. A small quantity of this native minium was found in the centre of a piece of cubic galena, accompanied with crystals of spar. This ingenious chemist promises to forward specimens of the mineral to his mineralogical friends, for their personal inspection.

LONDON INSTITUTION.

A general meeting of the proprietors of this institution was held on the 24th of April, at its house in the Old Jewry, to receive a report from the managers on a royal charter proposed to be applied for, to obtain for the society the rights of a corporate body.

A draft of the proposed charter was read to the meeting, which, after some discussion, was ordered to be printed for the use of the proprietors, and to be taken into their consideration at a future meeting.

The meeting then proceeded to elect, by ballot, the first officers, &c. of the institution, preparatory to the insertion of their names in the proposed charter; when the following gentlemen were elected:

President—For One Year.

Sir Francis Baring, bart. M.P.

Vice-Presidents—For Four Years.

Sir Richard Neave, bart. F.R.S. and F.A.S.

For Three Years.

Beeston Long, esq.

For Two Years.

George Hibbert, esq. F.L.S.

For one Year.

John Julius Angerstein, esq.

Managers—For Four Years.

Richard Clark, esq. F.A.S. chamberlain.

Rev. Matthew Raine, D.D. F.R.S. and F.A.S.

Richard Sharp, Esq. F.A.S.

John Smith, esq. M.P.

Henry Thornton, esq. M.P.

For

For Three Years.

Jeremiah Harman, esq.
Benjamin Harrison, esq. F.A.S.
William Hasledine Pepys, esq.
John Rennie, esq. F.R.S. F.A.S.
Robert Wigram, esq. F.R.S. F.A.S.

For Two Years.

Thomas Bodley, esq.
Charles Bosanquet, esq.
John Peter Hankey, esq.
Joseph Huddart, esq. F.R.S.
Job Matthew Raikes, esq.

For One Year.

Thomas Baring, esq.
Samuel Boddington, esq.
Nathaniel Bogle French, esq.
William Henry Hoare, esq.
Abraham Wilday Robarts, esq.

Visitors—For Four Years.

Sir John William Anderson, bart. M.P.
Henry Hoare, esq. F.A.S.
William Saunders, M.D. F.R.S. F.A.S.

For Three Years.

Sir William Blizard, F.R.S. F.A.S.
Sir Charles Price, bart. M.P.
Right hon. the lord mayor, James Shaw, esq.

For Two Years.

Thomson Bonar, esq.
Harvey Christian Combe, esq. M.P.
Sir Hugh Inglis, bart. M.P.

For One Year.

Charles Grant, esq. M.P.
Robert Hankey, esq.
William Manning, esq. M.P.

Auditors—For one Year.

Isaac Lyon Goldsmid, esq.
Thomas Hughan, esq.

John Inglis, esq.
Thomas Reid, esq.
William Salte, esq.

Treasurer—For one Year.

Sir William Curtis, bart. M.P.

Secretary—For one Year.

Samuel Woods, esq.

ROYAL COLLEGE OF SURGEONS, LONDON.

At the meeting of this body held on the 16th of April (instant), the Jacksonian prize for 1805 was adjudged to John Hyslop, esq. of Fenchurch-street, surgeon, for the best dissertation on "Injuries of the head from external violence."

There are two prize subjects for the present year:

1. The diseases of the joints, particularly the hip and knee, and the best mode of treatment.

2. Herniæ, and the best mode of treatment.

The dissertations are required to be delivered in before Christmas 1806.

SOCIETY OF ANTIQUARIES.

March 27. The right hon. the earl of Leicester, president, in the chair. An old painting was exhibited of three real portraits, placed around a table covered with cards and money, which was marked Edward VI. and Elizabeth: the three old gentlemen seem deeply engaged at play, and from their dress and character it was conjectured that the painting must have been executed shortly after the days of Elizabeth. The learned president seemed to have the most correct notions of the characters supposed to be delineated.

Drawings of some more of Mr. Weston's Greek coins were also exhibited.

April 17. The president in the chair.—Mr. Gough exhibited some excellent drawings of the church of St. Albans, displaying almost every style of architecture that has been used in this country.

A letter to the president from Mr. Morris at the Cape of Good Hope was read, containing accounts of the different English

English inscriptions found on stones there, relating the arrival and departure of sir Henry Middleton's fleet of East India merchantmen in 1604 and 1609, being the fourth voyage to India. The inscriptions were on stones placed in a conspicuous part of the shore, and designed merely as a notice for other English ships that might touch there, where and when any others might probably pass. The same writer mentions, that he saw the fluke of an anchor on the summit of Table mountain, a height at which no human effort could likely have placed it.

The right hon. lord Hutchinson was elected a fellow of this society, which was adjourned till the anniversary on the 23d of April.

On the 23d of April, being St. George's day, the society of antiquaries met at their apartments in Somerset-place, in pursuance of their statutes and charter of incorporation, to elect a president, council, and officers of the society for the year ensuing:—whereupon

George, earl of Leicester,	Craven Ord, esq.
William Bray, esq.	John, lord bishop of Salisbury,
Rev. John Brand, A.M.	John Willett Willett, esq.
Sir H. C. Englefield, bart.	Joseph Windham, esq. and
Rev. Dr. Hamilton,	Rev. T. Wm. Wrighte, A.M.
Samuel Lysons, esq.	

eleven of the council, were rechosen of the new council; and

Edward Astle, esq.	John Wilkinson, M.D.
James Bindley, esq.	Henry Norton Willis, esq.
Francis Douce, esq.	Hon. Brownlow, lord bishop of Winchester, and
Charles, duke of Norfolk,	Chas. Wat. Williams Wynne, esq.
Hon. John Peachy,	
John Sylvester, esq.	

ten of the other members of the society, were chosen of the new council, and they were severally declared to be the council for the year ensuing; and on a report made of the officers of the Society, it appeared that

George, earl of Leicester, was elected president,
William Bray, esq. treasurer,
Samuel Lysons, esq. director,

Rev. Thomas William Wrighte, A.M. secretary, and
Rev. John Brand, A.M. secretary for the year ensuing.

The society afterwards dined together at the Crown and Anchor tavern in the Strand, according to annual custom.

FRENCH NATIONAL INSTITUTE.

The class of history and of antient literature of the French National Institute lately held a meeting for the purpose of distributing the annual prizes.

One of the subjects which had been proposed was the following:

"To examine what was the administration of Egypt, from the conquest of that country by Augustus to the taking of Alexandria by the Arabs; to give an account of the changes which it experienced during that interval; the condition of the Egyptians; and to explain what was the condition of strangers domiciliated in Egypt, and particularly the Jews."

Of all the memoirs sent to the meeting, not one appeared to possess sufficient merit to obtain the prize; the subject was therefore again given out for the meeting in the month of July 1807.

The class then proposed, as the subject of a prize essay to be adjudged in July 1808, "to examine what has been the influence of the crusades upon the civil liberty of the people of Europe, upon their civilization, and upon the progress of their learning, commerce, and industry."

The prize offered is a gold medal of 1500 francs in value; and the discourse, which must be in French or Latin, must be given in before the 1st of April 1808.

IMPERIAL ACADEMY OF SCIENCES, LITERATURE, AND THE FINE ARTS, TURIN.

This academy, formerly the Royal Academy of Turin, which was new modelled in consequence of Piedmont being annexed to France, has published two quarto volumes of memoirs for the years 12 and 13. One of the volumes is appropriated to the labours of the Class of Literature and the Fine Arts; the other, compiled by the secretary, M. Vassali Eandi, mentions the changes that have taken place in the list of academicians, the various papers read at the meetings, and

and the presents made to the academy. After this follows an account of the labours of the academy up to 1805, which occupies 250 pages, and then the different memoirs, viz.

1. Description of a new portable barometer for measuring heights and depths, with observations made with it in the circles of Turin and Saluzzo: by Vassali Eandi.

2. Account of a waterspout that occurred in the territory of Revel, in the circle of Saluzzo, March 27, 1798; with remarks on the cause of the phænomenon: by the same.

3. On the different capacities for conducting heat, ascertained, by experiment, in different articles used for clothing: by J. Sennebier.

4. Of a new species of hawkweed (*crepis*), to which are added some cryptogamiæ of Piedmont: by J. Baptist Balbis.

A figure of this plant, which Mr. B. calls *crepis ambigua*, is given. Among the cryptogamiæ are the following new species: *mucor flosculentus*, *peziza amentacea*, *lichen nivalis*. These likewise are figured.

5. Experiments on the effects of the nitric and oxygenated muriatic acid, employed topically in the treatment of various diseases: by M. Rossi. Mr. R. gives an account of the cure of several gangrenous ulcers, venereal buboes, and even contagious carbuncles, effected by the application of these acids.

6. Meteorological observations made during the solar eclipse on the 30th of January 1805, at the observatory of Turin; with reflections on them: by Ant. Mar. Vassali Eandi.

7. On a species of cassia, that may be substituted for the senna of the shops: by M. Bellardi. This is the *cassia marilandica*, which Mr. B. would call *succedanea*, because, according to him, it may supply the place of *cassia lanceolata*.

8. Inquiries into the nature of the Galvanic fluid: by A. M. Vassali Eandi.

9. On the mines of plumbago in the departments of the Sture and Po: by M. Bonvoisin.

10. Attempts to improve nut oil: by the same. Mr. B. points out a method of purifying this oil, and rendering it as fit for lamps as other fine oils.

11. Examination of the action of the Galvanic fluid on different gases: by J. A. Giobert.

12. An anatomical and physiological essay on the lymphatic glands: by professor Rossi.

13. Solution of a problem depending on the theory of permutations and combinations: by professor Balbo.

14. Explanation of the circumstance of a fish being occasionally found with prickles in the river of the 27th military division: by M. Giorna. This fish is the *cyprinus idus*; the male only has prickles, and loses them after spawning-time.

15. A chemico-medical essay on the pulmonary consumption, by Jos. Hyac. Rizzetti. The principal subject of this essay is the nature of the matter expectorated.

The following papers are by foreign members:

1. Memoir on the use of varying the constant quantity in summing up equations with variable coefficients, by Dr. Brunacci.

2. A systematical enumeration of the coleoptera found in the territory of Saluzzo, with observations by Lau. Ponza. To this catalogue are annexed two plates, containing the following new species. *Coccinella numeralis*,—*c. obsoleta*,—*curculio spinosus*,—*c. dubius*,—*c. rugosus*,—*cerambyx præustus*,—*c. melanocephalus*,—*chrysomela melanocephala*,—*ch. variegata*,—*ch. pretiosa*,—*ch. luctuosa*,—*scarabæus rufescens*,—*cantharis impressifrons*,—*attelabus funereus*.—*dytiscus silphoides*,—*tenebrio rufus*,—*birrhus Rossii*,—*carabus attenuatus*,—*c. metallicus*,—*c. Rossii*,—*forficula bipunctata*,—*silpha sinuata*,—*s. scabra*.

3. On the motion of the hairs of the *hypnum adiantoides*, by Palamedas de Suffren. Parts endued with irritability had already been observed in the hairs of some mosses. Mr. De S. has found it in those of the *hypn. diant.* and describes all the singularities of the phænomenon. This paper is accompanied with a plate.

4. Of a resin employed by the bee in constructing its combs. By Fr. Mouxy Deloche.

5. Entomological observations; by Mr. Disderi. Mr. D. first

first sketches the history of the silkworm; and then proceeds to certain hymenoptera, chiefly of the genera *tenthredo, ichneumon, sphex,* and *vespa.*

6. Specimen of the fungi of the vale of Pisa, by Hugh Camino. The new species are figured on three plates. They are Agaricus elatior: a. miniatus: a. pezizoides: a. atrosanguineus: a. tricolor: Boletus scobinaceus: Helvella grandis: h. reflexa: h. inflata: Peziza ochracea: p. pyriformis: Reticularia rosea: Mucor fruticulosus.

7. Observations on the native gold found among sand, by Lew. Bossi, of Milan.

ACADEMY OF GENOA.

On the 26th of January last this academy had a public meeting, which was attended by the chancellor of the exchequer Le Brun, member of the French National Institute, and other distinguished inhabitants. M. Cotardo Solari opened the meeting with a discourse on the following subject: "That learning, when not regulated by wisdom, is productive of more injury than benefit to a state." The following prize questions were afterwards announced:

1. Has any other of the heavenly bodies, besides the sun and moon, any influence upon the meteorological appearances of the world?

2. Which is the best method of rendering the harbour of Genoa more secure and convenient for shipping?

3. Whether is it more advantageous for the inhabitants of the Ligurian republic to encourage the wine or oil trade? and what is the best method of cultivating the vine, and the plants which produce oil?

The prize for the best answer to each of these questions is a gold medal of 400 ducats in value: the second best discourse will be rewarded with a silver medal. The discourses must be given in on or before the 15th of November next.

VACCINE-POCK SOCIETY, AMSTERDAM.

This society lately held its annual sitting; when it appeared that out of 1800 persons inoculated with the cow-pock during the years 1804 and 5, not one had died, or taken the small-pox.

MEDICAL

MEDICAL SOCIETY OF LYONS.

This society has announced the following prize question: "What are the diagnostic and prognostic signs afforded by the state of the tongue, the lips, and the teeth, in acute and chronic disorders? and what advantages may result to practice from this discovery?"—The prize is a gold medal of 300 francs in value. The answer must be given in on or before August 1807.

ŒCONOMICAL SOCIETY OF THE BAHAMA ISLANDS.

This society has been presented by the British government with a hundred acres of ground near Nassau in New Providence, for the purpose of founding a botanical garden, and erecting a house wherein the society is to hold its meetings.

AMERICAN SOCIETIES.

Since Louisiana was added to the United States of North America, two learned societies have been formed in that district, at New Orleans and Natchez. That of the former is called "The Literary Society," and publishes a monthly journal, particularly intended to extend a knowledge of the country to other nations, and containing a collection of useful learning. The literary society at Natchez was established in Oct. 1803, and is called "The Missisippi Society for the Discovery and Extension of useful Knowledge;" it consists of between 30 and 40 members, has correspondents in the several united states, and special regulations of its own: both the above societies are sanctioned by the American government.

LIII. *Intelligence and Miscellaneous Articles.*

AEROSTATION.

Lisle, April 9

THE ninth aërial ascension announced by M. Mosment took place last Monday, (the 7th inst.) in the elegant rotunda of the circus of this city, in presence of an immense crowd of spectators. From day-break the aëronaut was busily occupi-

ed in the production of hydrogen gas; the sky was serene, and at mid-day the operation of filling the balloon was completed and every thing ready. M. Mosment leapt into the car; and upon a signal being given the balloon was set at liberty, and ascended very rapidly into the air amidst the shouts of the spectators. M. Mosment repeatedly waved his flag as he ascended, which was adorned with the imperial eagle. The wind was northerly, and the balloon was carried gently before it. At a certain height the aëronaut let go a parachute to which an animal was attached, and the experiment succeeded admirably. In the mean time the balloon continued to ascend, and appeared as if exactly above the town. At one o'clock it seemed to have encountered some adverse winds. Something red was then seen slowly descending, which was picked up, and found to be the flag which the aëronaut carried up with him. This however excited no disagreeable emotions, and all eyes were turned to the balloon, which soon ascended to such a height as to become invisible. The crowd then began to disperse, perfectly satisfied with the success of the experiment; but a rumour ran through the populace, that the dead body of a man had been found dreadfully mangled in one of the fossés of the town. This excited some inquiry; and upon inspecting the body, it was found to be that of the unfortunate aëronaut, but so covered with blood that it was with difficulty he was recognised.

This event gave rise to various conjectures; but it is generally supposed that the car was too shallow, and that M. Mosment had fallen asleep, as often happens to aëronauts when in certain regions of the air, and had been shaken out by the violence of the winds. It is possible also that one of the cords may have slipt, and that M. Mosment losing his balance had fallen out. The balloon has not yet been found; which prevents the real cause of the accident from being ascertained.

ICELAND.

The Danish government is occupied in the amelioration of the lot of the inhabitants of Iceland, a people removed to the confines of the polar circle, but interesting on account of

of the zeal with which they cultivated the sciences in the 10th and 11th centuries, and on account of the voyages they made to America. Iceland, almost ruined by various physical and political misfortunes, is about to be restored ; a regular city is building, to be called *Reykiavig* ; and it is already peopled by colonies of natives as well as strangers ; a free port invites the vessels of commerce ; and a college, where even the antient languages and natural history are taught, is in the full exercise of its functions.

NEW MUSICAL INSTRUMENT.

M. Diez, at Emmerich on the Rhine, has invented a new musical instrument, which he calls a *Melodion*. It occupies less room than an ordinary piano forte, and may be played on by any common performer, after a little practice. That which distinguishes it above all other instruments of the kind, is the circumstance of its producing the sounds of the clarionet, hautbois, and bassoon, in the softest piano or the most brilliant forte movements. The cost of such an instrument is about 40 louis d'ors.

ANTIQUITIES.

A letter from Petersburgh, of the 19th of March, mentions the discovery of two great cities in the Russian empire, of which no traces can be discovered in history ; one of them in the Isle of Taman in the Black Sea, the other in a district in Siberia.

CHEMICAL AFFINITIES.

An intelligent correspondent, Mr. Collard of Birmingham, the proprietor of a chemical laboratory in that neighbourhood, in which many articles necessary to the arts and manufactures of this kingdom are prepared on a very large scale, has communicated to us a new fact well worthy of the attention of chemists.

Contrary to the common tables of affinities, he finds that copper may be precipitated from its solution in the sulphuric acid by tin. All that is necessary to the success of the experiment is, that the solution be nearly at the boiling point, or actually boiling, when the tin is put into it. The tin made use of ought to be in filings or in leaf, or reduced

to thin fragments by pouring it, when in fusion, into cold water.

To the enlightened chemist we need not point out the experiments suggested by this new and curious fact, and the important results to which it may ultimately lead. If copper and tin, by a mere difference in the temperature of the solution, may be made mutually to precipitate each other, it is not impossible that the order of affinities with respect to other metals for the different acids may also be inverted by circumstances connected with temperature. Should any such results be obtained, they will be productive of incalculable advantages in many intricate cases of analysis. The different results obtained in apparently similar experiments, by equally accurate chemists, may perhaps have been owing in some cases to the existence of such a law as we now allude to.

MINERALOGY.

Dr. Aires, from New Jersey, who has lately explored several of the western counties of the State of North Carolina, in search of gold, reports, that he has discovered gold in branches and creeks in the counties of Cabarrus, Montgomery, and Randolph, in a north and north-east course, and in the county of Mecklenburgh, in a south and south-west direction from the first sound, where none had ever been discovered before, except in three or four branches near Reed's (the first discovery) in Cabarrus county. A few pieces of gold, intermixed with stone, have been found on the surface of the earth, and some ploughed up in most of the said counties. Several of the said water-courses contain considerable quantities of gold dust, which can be collected by washing the sand, after the first or common washing, with a machine proper for the purpose, and then by mixing mercury with the sand thus washed, which will unite with the particles of gold, and form an amalgam, whence the mercury may be driven off by heat.

A copper mine has been discovered on the estate of Hans Hamilton, esq., in the county of Dublin, which promises to be very productive. A company has been formed, and are already engaged in carrying it on.

TRAVELS.

TRAVELS.

The Polish Prince Alexander Sapieha has concluded his geological and archæological travels, and has returned to his estates in Poland for the purpose of enjoying there the treasures of art and science which he has collected. His longest stay was at Athens, where he was so fortunate as to preserve many of the last rays of the declining grandeur of antient Greece, in their native splendour.

The embassy to China left Maisnabischin, the first Chinese frontier town, in December, for Pekin. it consists of 120 persons; the rest of the suite returns, and some persons are already arrived in Russia. Six years ago, a Russian caravan, 200,000 rubles in value, going to Taschkent, was plundered by the Kingise. The chiefs have now engaged to make good the loss amicably.

The South American Society of Commerce at Petersburg have received intelligence that M. Resanow, the Russian ambassador to Japan, has not yet been admitted to an audience of the sovereign, but that he has been in other respects well received; the presents of which he was the bearer having been accepted, and an exchange made of the same description.

ANALYSIS OF WATERS.

Dr. Menuret, of Paris, has lately analysed the different waters in that neighbourhood.

The water of the Seine contains 5 grains $\frac{2}{3}$ of foreign matter in each pint. That of the river Yvette yields 7 grains $\frac{1}{3}$; that of the Arceuil, 7 grains $\frac{2}{3}$; that of Ville d'Avray, 9 grains $\frac{3}{4}$. Bristol water contains 14 grains $\frac{3}{4}$, &c.

Thus, of all the waters drunk at Paris, that of the Seine is the most salubrious, the purest, and the lightest.

SURGICAL INSTRUMENTS.

Drs. Faust and Hunold are preparing a work, in which they endeavour to prove, that, with the exception of the lancet used in inoculation, every surgical instrument ought to be dipped in oil before proceeding to the operation, and should also be heated to the temperature of the blood. By these

these precautions they contend that the pain of the operation is always diminished.

A REMARKABLE HEN.

For the three last summers, a hen, the property of Charles Ranken, at Auchinairn, parish of Cadder, has frequently laid eggs of an extraordinary size and weight. Within these few weeks, she has laid three eggs, each of which measures in diameter 6½ inches by 7½, and weighs fully 3½ oz.; and generally, on the day before she lays the large egg, she lays an egg of an ordinary size.

LECTURES.

Mr. Taunton's next Course of Lectures on Anatomy, Physiology, and Surgery, will commence on Saturday the 24th of May, at 8 o'clock in the evening, at his house in Greville Street, Hatton Garden.

LIST OF PATENTS FOR NEW INVENTIONS.

To Samuel Miller, of the parish of Saint Pancras, in the county of Middlesex; for various improvements in the working of coal, tin, lead, and other mines, by which there will be a great saving of fuel and labour, and many accidents prevented. Dated April 1, 1806.

To James Keir, of West Bromwich, in the county of Stafford, esq.; for his improved method of manufacturing white lead. Dated April 3, 1806.

To William Henry Lassalle, of the city of Bristol, apothecary; for certain improvements in soap. Dated April 5, 1806.

To James Kay, of Preston, in the county of Lancaster, machine-maker; for improvements upon Thomas Johnson's patent machine for dressing cotton, silk, and other goods by power. Dated April 17, 1806.

To Thomas James Plucknett, of the parish of Christ Church, in the county of Surry, agricultural machine maker; for a machine for dibbling and drilling all kinds of grain and pulse. Dated April 17, 1806.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand;
For April 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. 11 o'Clock, Night.	Thermometer. Noon.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
March 27	46°	47°	42°	28·85	12°	Cloudy
28	41	42	40	·89	16	Cloudy
29	39	43	39	·83	9	Cloudy
30	39	46	38	·90	23	Cloudy
31	40	47	38	30·06	12	Cloudy
April 1	36	42	34	·30	21	Cloudy
2	35	42	34	·22	13	Cloudy
3	35	41	35	·22	21	Cloudy
4	35	40	36	·10	7	Cloudy
5	39	48	41	29·95	22	Fair
6	46	51	42	·90	37	Fair
7	47	55	39	·86	34	Fair
8	39	55	42	·90	34	Fair
9	46	56	40	·72	25	Fair
10	44	46	38	·51	14	Cloudy
11	35	41	34	·55	20	Cloudy
12	33	34	29	·65	22	Cloudy
13	33	34	33	·35	0	Snow
14	35	38	34	·58	0	Snow
15	34	39	34	·80	23	Cloudy
16	34	49	40	30·28	47	Fair
17	40	55	45	·40	47	Fair
18	46	58	49	·36	18	Cloudy
19	49	59	52	·32	7	Cloudy
20	52	61	52	·31	17	Fair
21	52	57	48	·30	5	Cloudy
22	49	55	45	·18	0	Showery
23	44	49	43	·22	30	Fair
24	44	51	46	·28	10	Cloudy
25	46	51	40	·25	18	Cloudy
26	40	44	38	·34	31	Cloudy

N. B. The barometer's height is taken at noon

LIV. *On Transit Instruments.* *By* Ez. Walker, *Esq.*

To Mr. Tilloch.

SIR,

Astronomers have, in all ages, been very attentive to the construction of instruments for determining the exact time when the sun, or any other celestial luminary, passes over the meridian. The best instrument that has yet been invented for this purpose, is the transit telescope; but the difficulty of procuring a proper foundation to fix it upon, and the expense of fitting it up, must render its use extremely limited. Clocks and watches may be regulated for occasional observations, by means of equal altitudes of the sun or stars, taken either with an astronomical quadrant or a Hadley's sextant: but these methods are very inconvenient for keeping the daily rate of a time-keeper, in consequence of the length of the calculations, and the time that is required for taking the observations. Hence it may be presumed, that an instrument for finding the exact time of noon in an easy manner, is still wanted, to supply the place of the transit telescope.

The method used in former ages for resolving this problem has, perhaps, been too much neglected since the time that the science of optics began to receive so many improvements. The method to which I allude is that of finding a meridian line by means of the sun's rays transmitted through a small circular aperture made in a piece of metal. In this manner, a kind of transit instrument may be constructed, at a small expense, in many situations that afford no foundation for a transit telescope, and to a greater degree of accuracy than may, perhaps, be generally supposed.

Having, some years ago, drawn a line by this method, and being desirous to know how far it deviated from the true meridian, I took 14 equal altitudes of the sun from the surface of a fluid with one of Mr. Stancliffe's best 12-inch sextants. The time of noon, derived from the mean of

those observations, differed only 2·27″ from the time that the sun passed over the meridian line on the same day.

But in order to observe the sun's transit over the meridian with greater precision, I drew two other lines on each side of the meridian line, which were parallel to it, and at such distances, that the interval of time taken by one of the limbs of the sun's image was about 40 seconds in passing from one line to another, when the sun was near the equator. Those who are at all acquainted with the use of the transit telescope, will be at no loss to comprehend the use of these lines; and those who may want directions for the use of that instrument, may consult a treatise on the method of finding the longitude at sea by time-keepers, written by the late Mr. William Wales. To those directions I shall only add, that the room in which the lines are drawn must not be made too dark during the time of observing, as it is necessary to see the lines before they are illuminated by the sun's image.

When the solar rays, that flow through a small circular aperture into a room, are received upon the floor, an elliptical image will be formed, consisting of a bright central image, and a penumbra surrounding it. This penumbra renders the contact of the sun's image with a line less certain: but, notwithstanding this inconvenience, I am convinced that an experienced observer, under favourable circumstances, will generally determine the time of the sun's passage over the meridian line to one-fifth of a second, or less, when the distance between the aperture and the sun's image on the floor measures about 12 feet. Further particulars respecting the construction and use of instruments of this description will be submitted to your inspection at some future opportunity.

I am, sir,

Your most humble servant,

Ez. Walker.

Lynn, May 8, 1806.

LV. *Account*

LV. *Account of a Series of Experiments, showing the Effects of Compression in modifying the Action of Heat. By Sir* James Hall, *Bart. F. R. S. Edin.*

[Continued from p. 203.]

IV. *Experiments in Gun-Barrels resumed.—The Vertical Apparatus applied to them.—Barrels bored in solid Bars.—Old Sable Iron.—Fusion of the Carbonate of Lime.—Its Action on Porcelain.—Additional Apparatus required in consequence of that Action.—Good Results; in particular, four Experiments illustrating the Theory of Internal Calcination, and showing the Efficacy of the Carbonic Acid as a Flux.*

Since I found that, with porcelain tubes, I could neither confine the carbonic acid entirely, nor expose the carbonate in them to strong heats, I at last determined to lay them aside, and return to barrels of iron, with which I had formerly obtained some good results, favoured, perhaps, by some accidental circumstances.

On the 12th of February 1803, I began a series of experiments with gun-barrels, resuming my former method of working with the fusible metal, and with lead; but altering the position of the barrel from horizontal to vertical, the breech being placed upwards during the action of heat on the carbonate. This very simple improvement has been productive of advantages no less remarkable than in the case of the tubes of porcelain. In this new position, the included air, quitting the air tube on the fusion of the metal, and rising to the breech, is exposed to the greatest heat of the furnace, and must therefore re-act with its greatest force; whereas, in the horizontal position, that air might go as far back as the fusion of the metal reached, where its elasticity would be much feebler. The same disposition enabled me to keep the muzzle of the barrel plunged, during the action of heat, in a vessel filled with water; which contributed very much both to the convenience and safety of these experiments.

In this view, making use of the brick furnace with the

vertical muffle, already described in page 197, I ordered a pit (*aaa*, fig. 20.) to be excavated under it, for the purpose of receiving a water vessel. This vessel (represented separately, fig. 21.) was made of cast iron; it was three inches in diameter, and three feet deep; and had a pipe (*de*) striking off from it at right angles, four or five inches below its rim, communicating with a cup (*ef*) at the distance of about two feet. The main vessel being placed in the pit (*aa*) directly below the vertical muffle, and the cup standing clear of the furnace, water poured into the cup flowed into the vessel, and could thus conveniently be made to stand at any level. (The whole arrangement is represented in fig. 20.) The muzzle of the barrel (*g*) being plunged into the water, and its breech (*h*) reaching up into the muffle, as far as was found convenient, its position was secured by an iron chain (*gf*). The heat communicated downwards generally kept the surface of the water (at *c*) in a state of ebullition; the waste thus occasioned being supplied by means of the cup, into which, if necessary, a constant stream could be made to flow.

As formerly, I rammed the carbonate into a tube of porcelain, and placed it in a cradle of iron, along with an air-tube and a pyrometer; the cradle being fixed to a rod of iron, which rod I now judged proper to make as large as the barrel would admit, in order to exclude as much of the fusible metal as possible; for the expansion of the liquid metal being in proportion to the quantity heated, the more that quantity could be reduced, the less risk there was of destroying the barrels.

In the course of practice, a simple mode occurred of removing the metal and withdrawing the cradle: it consisted in placing the barrel with its muzzle downwards, so as to keep the breech above the furnace and cold, while its muzzle was exposed to strong heat in the muffle. In this manner the metal was discharged from the muzzle, and the position of the barrel being lowered by degrees, the whole metal was removed in succession, till at last the cradle and its contents became entirely loose. As the metal was delivered it was received in a crucible filled with water, standing on a plate of

of iron placed over the pit, which had been used, during the first stage of the experiment, to contain the water vessel. It was found to be of service, especially where lead was used, to give much more heat to the muzzle than simply what was required to liquefy the metal it contained; for when this was not done, the muzzle growing cold as the breech was heating, some of the metal delivered from the breech was congealed at the muzzle, so as to stop the passage.

According to this method, many experiments were made in gun-barrels, by which some very material steps were gained in the investigation.

On the 24th of February I made an experiment with spar and chalk; the spar being placed nearest to the breech of the barrel, and exposed to the greatest heat, some baked clay intervening between the carbonates. On opening the barrel, a long continued hissing noise was heard. The spar was in a state of entire calcination; the chalk, though crumbling at the outside, was uncommonly hard and firm in the heart. The temperature had risen to 32°.

In this experiment we have the first clear example, in iron barrels, of what I call *internal calcination*; that is to say, where the carbonic acid, separated from the earthy basis, has been accumulated in cavities within the barrel. For, subsequently to the action of strong heat, the barrel had been completely cooled; the air, therefore, introduced by means of the air tube, must have resumed its original bulk, and by itself could have no tendency to rush out; the heat employed to open the barrel being barely sufficient to soften the metal. Since, then, the opening of the barrel was accompanied by the discharge of elastic matter in great abundance, it is evident that this must have proceeded from something superadded to the air originally included, which could be nothing but the carbonic acid of the carbonate. It follows, that the calcination had been, in part at least, internal; the separation of the acid from the earthy matter being complete where the heat was strongest, and only partial where the intensity was less.

The chemical principles stated in a former part of this paper, authorised us to expect a result of this kind. As

heat, by increasing the volatility of the acid, tended to separate it from the earth, we had reason to expect, that, under the same compression, but in different temperatures, one portion of the carbonate might be calcined, and another not: and that the least heated of the two would be least exposed to a change, not only from want of heat, but likewise in consequence of the calcination of the other mass; for the carbonic acid disengaged by the calcination of the hottest of the two, must have added to the elasticity of the confined elastic fluid, so as to produce an increase of compression. By this means the calcination of the coldest of the two might be altogether prevented, and that of the hottest might be hindered from making any further advancement. This reasoning seemed to explain the partial calcinations which had frequently occurred where there was no proof of leakage; and it opened some new practical views in these experiments, of which I availed myself without loss of time. If the internal calcination of one part of an inclosed mass promotes the compression of other masses included along with it, I conceived that we might forward our views very much by placing a small quantity of carbonate, carefully weighed, in the same barrel with a large quantity of that substance; and by arranging matters so that the small fiducial part should undergo a moderate heat, while a stronger heat, capable of producing internal calcination, should be applied to the rest of the carbonate. In this manner I made many experiments, and obtained results which seemed to confirm this reasoning, and which were often very satisfactory, though the heat did not always exert its greatest force where I intended it to do so.

On the 28th of February I introduced some carbonate, accurately weighed, into a small porcelain tube, placed within a larger one, the rest of the large tube being filled with pounded chalk; these carbonates, together with some pieces of chalk, placed along with the large tube in the cradle, weighing in all 195·7 grains. On opening the barrel, air rushed out with a long-continued hissing noise. The contents of the little tube were lost by the intrusion of some borax which had been introduced over the silex in order to exclude

exclude the fusible metal. But the rest of the carbonate, contained in the large tube, came out in a fine state, being porous and frothy throughout; sparkling every where with facettes, the angular form of which was distinguishable in some of the cavities by help of a lens: in some parts the substance exhibited the rounding of fusion; in many it was in a high degree transparent. It was yellow towards the lower end, and at the other almost colourless. At the upper end the carbonate seemed to have united with the tube, and at the places of contact to have spread upon it, the union having the appearance of a mutual action. The general mass of carbonate effervesced in acid violently, but the thin stratum immediately contiguous to the tube, feebly, if at all.

On the 3d of March I introduced into a very clean tube of porcelain 36·8 of chalk. The tube was placed in the upper part of the cradle, the remaining space being filled with two pieces of chalk, cut for the purpose; the uppermost of these being excavated, so as to answer the purpose of an air-tube. The pieces thus added were computed to weigh about 300 grains. There was no pyrometer used, but the heat was guessed to be about 30°. After the barrel had stood during a few minutes in its delivering position, the whole lead, with the rod and cradle, were thrown out with a smart report, and with considerable force. The lowermost piece of chalk had scarcely been acted upon by heat. The upper part of the other piece was in a state of marble, with some remarkable facettes. The carbonate in the little tube had shrunk very much during the first action of heat, and had begun to sink upon itself by a further advancement towards liquefaction. The mass was divided into several cylinders, lying confusedly upon each other; this division arising from the manner in which the pounded chalk was rammed into the tube in successive portions. In several places, particularly at the top, the carbonate was very porous, and full of decided air-holes, which could not have been formed but in a soft substance; the globular form and shining surface of all these cavities clearly indicating fusion. The substance was semi-transparent; in some places yellow, and in some colourless. When broken, the solid parts

showed a saline fracture, composed of innumerable facettes. The carbonate adhered, from end to end, to the tube, and incorporated with it, so as to render it impossible to ascertain what loss had been sustained. In general, the line of contact was of a brown colour; yet there was no room for suspecting the presence of any foreign matter, except, perhaps, from the iron rod which was used in ramming down the chalk. But, in subsequent experiments, I have observed the same brown or black colour at the union of the carbonate with the porcelain tubes, where the powder had been purposely rammed with a piece of wood; so that this colour, which has occurred in almost every similar case, remains to be accounted for. The carbonate effervesced violently with acid; the substance in contact with the tube doing so, however, more feebly than in the heart, leaving a copious deposit of white sandy matter, which is doubtless a part of the tube, taken up by the carbonate in fusion.

On the 24th of March I made a similar experiment in a stout gun-barrel, and took some care, after the application of heat, to cool the barrel slowly, with a view to crystallization. The whole mass was found in a fine state, and untouched by the lead; having a semi-transparent and saline structure, with various facettes. In one part I found the most decided crystallization I had obtained, though of a small size: owing to its transparency it was not easily visible till the light was made to reflect from the crystalline surface, which then produced a dazzle, very observable by the naked eye: when examined by means of a lens, it was seen to be composed of several plates, broken irregularly in the fracture of the specimen, all of which are parallel to each other, and reflect under the same angle, so as to unite in producing the dazzle. This structure was observable equally well in both parts of the broken specimen. In a former experiment, as large a facette was obtained in a piece of solid chalk; but this result was of more consequence, as having been produced from chalk previously pounded.

The foregoing experiments proved the superior efficacy of iron vessels over those of porcelain, even where the thickness was not great; and I persevered in making a great many experiments

experiments with gun-barrels, by which I occasionally obtained very fine results: but I was at last convinced that their thickness was not sufficient to ensure regular and steady success. For this purpose it appeared proper to employ vessels of such strength, as to bear a greater expansive force than was just necessary; since, occasionally (owing to our ignorance of the relation between the various forces of expansion, affinity, tenacity, &c.), much more strain has been given to the vessels than was requisite. In such cases barrels have been destroyed, which, as the results have proved, had acted with sufficient strength during the first stages of the experiments, though they had been unable to resist the subsequent overstrain. Thus my success with gun-barrels depended on the good fortune of having used a force no more than sufficient to constrain the carbonic acid, and enable it to act as a flux on the lime. I therefore determined to have recourse to iron barrels of much greater strength, and tried various modes of construction.

I had some barrels executed by wrapping a thick plate of iron round a mandrel, as is practised in the formation of gun-barrels; and likewise by bringing the two flat sides together, so as to unite them by welding. These attempts, however, failed. I next thought of procuring bars of iron, and of having a cavity bored out of the solid, so as to form a barrel. In this manner I succeeded well. The first barrel I tried in this way was of small bore, only half an inch: its performance was highly satisfactory, and such as to convince me, that the mode now adopted was the best of any that I had tried. Owing to the smallness of the bore, a pyrometer could not be used internally, but was placed upon the breech of the barrel, as it stood in the vertical muffle. In this position it was evidently exposed to a much less heat than the fiducial part of the apparatus, which was always placed, as nearly as could be guessed, at the point of greatest heat.

On the 4th of April an experiment was made in this way with some spar, the pyrometer on the breech giving 33°. The spar came out clean, and free from any contamination, adhering

adhering to the inside of the porcelain tube: it was very much shrunk, still retaining a cylindrical form, though bent by partial adhesions. Its surface bore scarcely any remains of the impression taken by the powder on ramming it into the tube: it had, to the naked eye, the roughness and semi-transparency of the pith of a rush stripped of its outer skin. By the lens this same surface was seen to be glazed all over, though irregularly, showing here and there some air-holes. In fracture it was semi-transparent, more vitreous than crystalline, though having a few facettes: the mass was seemingly formed of a congeries of parts, in themselves quite transparent; and, at the thin edges, small pieces were visible of perfect transparency. These must have been produced in the fire; for the spar had been ground with water, and passed through sieves, the same with the finest of those used at Etruria, as described by Mr. Wedgewood, in his paper on the construction of his pyrometer.

With the same barrel I obtained many interesting results, giving as strong proofs of fusion as in any former experiments; with this remarkable difference, that, in these last, the substance was compact, with little or no trace of frothing. In the gun-barrels where fusion had taken place, there had always been a loss of 4 or 5 per cent., connected, probably, with the frothing. In these experiments, for a reason soon to be stated, the circumstance of weight could not be observed; but appearances led me to suppose, that here the loss had been small, if any.

On the 6th of April I made another experiment with the square barrel, whose thickness was now much reduced by successive scales, produced by oxidation, and in which a small rent began to appear externally, which did not, however, penetrate to the bore. The heat rose high, a pyrometer on the breech of the barrel giving 37°. On removing the metals, the cradle was found to be fixed, and was broken in the attempts made to withdraw it. The rent was much widened externally: but it was evident that the barrel had not been laid open, for part of the carbonate was in a state of saline marble; another was hard and white, without any saline

saline grains, and scarcely effervesced in acid. It was probably quicklime, formed by internal calcination, but in a state that has not occurred in any other experiment.

The workman whom I employed to take out the remains of the cradle, had cut off a piece from the breech of the barrel, three or four inches in length. As I was examining the crack which was seen in this piece, I was surprised to see the inside of the barrel lined with a set of transparent and well defined crystals, of small size, yet visible by the naked eye. They lay together in some places, so as to cover the surface of the iron with a transparent coat; in others they were detached, and scattered over the surface. Unfortunately, the quantity of this substance was too small to admit of much chemical examination; but I immediately ascertained that it did not in the least effervesce in acid, nor did it seem to dissolve in it. The crystals were in general transparent and colourless, though a few of them were tinged seemingly with iron. Their form was very well defined, being flat, with oblique angles, and bearing a strong resemblance to the crystals of the lamellated stylbite of Haüy. Though made above two years ago, they still retain their form and transparency unchanged. Whatever this substance may be, its appearance, in this experiment, is in the highest degree interesting, as it seems to afford an example of the mode in which Dr. Hutton supposes many internal cavities to have been lined, by the sublimation of substances in a state of vapour, or held in solution by matters in a gaseous form. For, as the crystals adhered to a part of the barrel, which must have been occupied by air during the action of heat, it seems next to certain that they were produced by sublimation.

The very powerful effects produced by this last barrel, the size of which (reduced, indeed, by repeated oxidation) was not above an inch square, made me very anxious to obtain barrels of the same substance, which, being made of greater size, ought to afford results of extreme interest. I found, upon inquiry, that this barrel was not made of Swedish iron, as I at first supposed, but of what is known by the name of *old sable*, from the figure of a sable stamped upon the bars; that

that being the armorial badge of the place in Siberia where this iron is made*.

A workman explained to me some of the properties of different kinds of irons, most interesting in my present pursuit; and he illustrated what he said by actual trial. All iron, when exposed to a certain heat, crushes and crumbles under the hammer; but the temperature in which this happens, varies with every different species. Thus, as he showed me, cast iron crushes in a dull red heat, or perhaps about 15° of Wedgewood; steel in a heat, perhaps, of 30°; Swedish iron, in a bright white heat, perhaps of 50° or 60°; old sable itself likewise yields, but in a much higher heat, perhaps of 100°. I merely guessed at these temperatures; but I am certain of this, that in a heat similar to that in which Swedish iron crumbled under the hammer, the old sable withstood a strong blow, and seemed to possess considerable firmness. It is from a knowledge of this quality that the blacksmith, when he first takes his iron from the forge, and lays it on the anvil, begins by very gentle blows, till the temperature has sunk to the degree in which the iron can bear the hammer. I observed, as the strong heat of the forge acted on the Swedish iron, that it began to boil at the surface, clearly indicating the discharge of some gaseous matter; whereas, the old sable, in the same circumstances, acquired the shining surface of a liquid, and melted away without any effervescence. I procured, at this time, a considerable number of bars of that iron, which fully answered my expectations.

By the experiments last mentioned, a very important point was gained in this investigation; the complete fusibility of the carbonate under pressure being thereby established. But from this very circumstance a necessity arose of adding some new devices to those already described: for the carbonate, in fusion, spreading itself on the inside of the tube containing it, and the two uniting firmly together, so as to be quite inseparable, it was impossible, after the experiment, to ascertain the weight of the carbonate by any

* I was favoured with this account of it by the late professor Robison.

method

method previously used. I therefore determined in future to adopt the following arrangement.

A small tube of porcelain (*i k*, fig. 23.) was weighed by means of a counterpoise of sand, or granulated tin; then the carbonate was firmly rammed into the tube, and the whole weighed again: thus the weight of the carbonate, previous to the experiment, was ascertained. After the experiment, the tube, with its contents, was again weighed; and the variation of weight obtained, independently of any mutual action that had taken place between the tube and the carbonate. The balance which I used, turned, in a constant and steady manner, with one hundredth of a grain. When pounded chalk was rammed into this tube, I generally left part of it free, and in that space laid a small piece of lump chalk (*i*), dressed to a cylinder, with the ends cut flat and smooth; and I usually cut a letter on each end, the more effectually to observe the effects produced by heat upon the chalk; the weight of this piece of chalk being always estimated along with that of the powder contained in the tube. In some experiments I placed a cover of porcelain on the muzzle of the little tube, (this cover being weighed along with it,) in order to provide against the case of ebullition: but as that did not often occur, I seldom took the trouble of this last precaution.

It was now of consequence to protect the tube, thus prepared, from being touched during the experiment by any substance, above all by the carbonate of lime, which might adhere to it, and thus confound the appreciation by weight. This was provided for as follows: The small tube (fig. 23. *i k*), with its pounded carbonate (*k*), and its cylinder of lump chalk (*i*), was dropt into a large tube of porcelain (*p k*, fig. 24). Upon this a fragment of porcelain (*l*), of such a size as not to fall in between the tubes, was laid. Then a cylinder of chalk (*m*) was dressed, so as nearly to fit and fill up the inside of the large tube, one end of it being rudely cut into the form of a cone. This mass being then introduced, with its cylindrical end downwards, was made to press upon the fragment of porcelain (*l*). I then dropped into the space (*n*), between the conical part of this mass and

and the tube, a set of fragments of chalk, of a size beyond what could possibly fall between the cylindrical part and the tube, and pressed them down with a blunt tool, by which the chalk being at the same time crushed and rammed into the angle, was forced into a mass of some solidity, which effectually prevented any thing from passing between the large mass of chalk and the tube. In practice, I have found this method always to answer, when done with care. I covered the chalk, thus rammed, with a stratum of pounded flint (*o*), and that again with pounded chalk (*p*) firmly rammed. In this manner I filled the whole of the large tube with alternate layers of silex and chalk; the muzzle being always occupied with chalk, which was easily pressed into a mass of tolerable firmness, and, suffering no change in very low heats, excluded the fusible metal in the first stages of the experiment.

The large tube, thus filled, was placed in the cradle, sometimes with the muzzle upwards, and sometimes the reverse. I have frequently altered my views as to that part of the arrangement, each mode possessing peculiar advantages and disadvantages. With the muzzle upwards, (as shown in fig. 24. and 25.) the best security is afforded against the intrusion of the fusible metal; because the air, quitting the air-tube in the working position, occupies the upper part of the barrel; and the fusible metal stands as a liquid (at *q*, fig. 25.) below the muzzle of the tube, so that all communication is cut off between the liquid metal and the inside of the tube. On the other hand, by this arrangement, the small tube, which is the fiducial part of the apparatus, is placed at a considerable distance from the breech of the barrel, so as either to undergo less heat than the upper part, or to render it necessary that the barrel be thrust high into the muffle.

With the muzzle of the large tube downwards, the inner tube is placed (as shown in fig. 22.) so as still to have its muzzle upwards, and in contact with the breech of the large tube. This has the advantage of placing the small tube near to the breech of the barrel: and though there is here less security against the intrusion of liquid metal, I have found that a point of little consequence; since, when the

the experiment is a good one, and that the carbonic acid has been well confined, the intrusion seldom takes place in any position. In whichever of the two opposite positions the large tube was placed, a pyrometer was always introduced, so as to lie as near as possible to the small tube. Thus, in the first-mentioned position, the pyrometer was placed immediately below the large tube, and, in the other position, above it; so that, in both cases, it was separated from the carbonate by the thickness only of the two tubes.

Much room was unavoidably occupied by this method, which necessarily obliged me to use small quantities of carbonate; the subject of experiment seldom weighing more than 10 or 12 grains, and in others far less *.

On the 11th of April 1803, with a barrel of old sable iron, having a bore of 0·75 of an inch, I made an experiment in which all these arrangements were put in practice. The large tube contained two small ones; one filled with spar, and the other with chalk. I conceived that the heat had risen to 33°, or somewhat higher. On melting the metals, the cradle was thrown out with considerable violence. The pyrometer, which, in this experiment, had been placed within the barrel, to my astonishment indicated 64°. Yet all was sound. The two little tubes came out quite clean and uncontaminated. The spar had lost 17·0 per cent.: the chalk 10·7 per cent. The spar was half sunk down, and run against the side of the little tube: its surface was shining, its texture spongy, and it was composed of a transparent and jelly-like substance: the chalk was entirely in a state of froth. This experiment extends our power of action, by showing, that compression, to a considerable degree, can be carried on in so great a heat as 64°. It seems likewise to prove, that in some of the late experiments with

* I measured the capacity of the air-tubes by means of granulated tin, acting as a fine and equal sand. By comparing the weight of this tin with an equal bulk of water, I found that a cubic inch of it weighed 1330·6 grains, and that each grain of it corresponded to 0·00075 of a cubic inch. From these data I was able, with tolerable accuracy, to gauge a tube by weighing the tin required to fill it.

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the square barrel, the heat had been much higher than was supposed at the time, from the indication of the pyrometer placed on the breech of the barrel; and that in some of them, particularly in the last, it must have risen at least as high as in the present experiment.

On the 21st of April 1805, a similar experiment was made with a new barrel, bored in a square bar of old sable of about two inches and a half in diameter, having its angles merely rounded, the inner tube being filled with chalk. The heat was maintained during several hours, and the furnace allowed to burn out during the night. The barrel had the appearance of soundness, but the metals came off quietly, and the carbonate was entirely calcined, the pyrometer indicating 63°. On examination, and after beating off the smooth and even scale of oxide peculiar to the old sable, the barrel was found to have yielded in its peculiar manner; that is, by the opening of the longitudinal fibres. This experiment, notwithstanding the failure of the barrel, was one of the most interesting I had made, since it afforded proof of complete fusion. The carbonate had boiled over the lips of the little tube, standing, as just described, with its mouth upwards, and had run down to within half an inch of its lower end: most of the substance was in a frothy state, with large round cavities, and a shining surface; in other parts it was interspersed with angular masses, which have evidently been surrounded by a liquid in which they floated. It was harder, I thought, than marble; giving no effervescence, and not turning red like quicklime in nitric acid, which seemed to have no effect upon it in the lump. It was probably a compound of quicklime with the substance of the tube.

With the same barrel repaired, and with others like it, many similar experiments were made at this time with great success; but to mention them in detail, would amount nearly to a repetition of what has been said. I shall take notice of only four of them, which, when compared together, throw much light on the theory of these operations, and likewise seem to establish a very important principle in geology.

geology. These four experiments differ from each other only in the heat employed, and in the quantity of air introduced.

The first of these experiments was made on the 27th of April 1803, in one of the large barrels of old sable, with all the above-mentioned arrangements. The heat had risen, contrary to my intention, to 78° and 79°. The tubes came out uncontaminated with fusible metal, and every thing bore the appearance of soundness. The contents of the little tube, consisting of pounded chalk, and of a small piece of lump chalk, came out clean, and quite loose, not having adhered to the inside of the tube in the smallest degree. There was a loss of 41 per cent., and the calcination seemed to be complete; the substance, when thrown into nitric acid, turning red, without effervescence, at first, though, after lying a few minutes, some bubbles appeared. According to the method followed in all these experiments, and lately described at length, (and shown in fig. 24. and 25.) the large tube was filled over the small one, with various masses of chalk, some in lump, and some rammed into it in powder; and in the cradle there lay some pieces of chalk, filling up the space, so that in the cradle there was a continued chain of carbonate of four or five inches in length. The substance was found to be less and less calcined, the more it was removed from the breech of the barrel, where the heat was greatest. A small piece of chalk, placed at the distance of half an inch from the small tube, had some saline substance in the heart, surrounded and intermixed with quicklime, distinguished by its dull white. In nitric acid this substance became red, but effervesced pretty briskly; the effervescence continuing till the whole was dissolved. The next portion of chalk was in a firm state of limestone; and a lump of chalk in the cradle was equal in perfection to any marble I have obtained by compression; the two last-mentioned pieces of chalk effervescing with violence in the acid, and showing no redness when thrown into it. These facts clearly prove that the calcination of the contents of the small tube had been internal, owing to the violent heat

which had separated its acid from the most heated part of the carbonate, according to the theory already stated. The soundness of the barrel was proved by the complete state of those carbonates which lay in less heated parts. The air-tube in this experiment had a capacity of 0·29, nearly one-third of a cubic inch.

The second of these experiments was made on the 29th of April, in the same barrel with the last, after it had afforded some good results. The air-tube was reduced to one-third of its former bulk, that is, to one-tenth of a cubic inch. The heat rose to 60°. The barrel was covered externally with a black spongy substance, the constant indication of failure; and a small drop of white metal made its appearance. The cradle was removed without any explosion or hissing. The carbonates were entirely calcined. The barrel had yielded, but had resisted well at first; for the contents of the little tube were found in a complete state of froth, and running with the porcelain.

The third experiment was made on the 30th of April in another similar barrel. Every circumstance was the same as in the two last experiments, only that the air-tube was now reduced to half its last bulk, that is, to one-twentieth of a cubic inch. A pyrometer was placed at each end of the large tube. The uppermost gave 41°, the other only 15°. The contents of the inner tube had lost 16 per cent., and were reduced to a most beautiful state of froth, not very much injured by the internal calcination, and indicating a thinner state of fusion than had appeared.

The fourth experiment was made on the 2d of May, like the rest in all respects, with a still smaller air-tube, of 0·0318, being less than one-thirtieth of a cubic inch. The upper pyrometer gave 25°, and the under one 16°. The lowest masses of carbonate were scarcely affected by the heat: the contents of the little tube had lost 2·9 per cent.; both the lump and the pounded chalk were in a fine saline state, and in several places had run and spread upon the inside of the tube, which I had not expected to see in such a low heat. On the upper surface of the chalk rammed into the

the little tube, which after its introduction had been wiped smooth, were a set of white crystals, with shining facettes, large enough to be distinguished by the naked eye, and seeming to rise out of the mass of carbonate. I likewise observed that the solid mass on which these crystals stood was uncommonly transparent.

In these four experiments the bulk of the included air was successively diminished, and by that means its elasticity increased. The consequence was, that in the first experiment, where that elasticity was the least, the carbonic acid was allowed to separate from the lime, in an early stage of the rising heat, lower than the fusing point of the carbonate, and complete internal calcination was effected. In the second experiment, the elastic force being much greater, calcination was prevented, till the heat rose so high as to occasion the entire fusion of the carbonate, and its action on the tube, before the carbonic acid was set at liberty by the failure of the barrel. In the third experiment, with still greater elastic force, the carbonate was partly constrained, and its fusion accomplished, in a heat between 41° and 15°. In the last experiment, where the force was strongest of all, the carbonate was almost completely protected from decomposition by heat, in consequence of which it crystallized and acted on the tube in a temperature between 25° and 16°. On the other hand, the efficacy of the carbonic acid as a flux on the lime, and in enabling the carbonate to act as a flux on other bodies, was clearly evinced; since the first experiment proved, that quicklime, by itself, could neither be melted, nor act upon porcelain, even in the violent heat of 79°; whereas, in the last experiment, where the carbonic acid was retained, both of these effects took place in a very low temperature.

[To be continued.]

LVI. *Observations and Advices respecting Improvements compatible with the quickest Manufacture of Muscovado Sugar, and conducive to the Melioration of the Rum. By* Bryan Higgins, *M. D.**

Sound cane-juice consists of water, sugar, deliquescent sweet, herbaceous matter, carbonic acid, and melasses acid: and some juices contain variable quantities of other ingredients, which are not yet to be noticed.

In these pharmaceutic ingredients subsist the primary or chemical principles of many vegetable acids. But experience shows, that the composition of attractive forces, resulting from such proportions of the principles as take place in the recent juice, tend chiefly to the formation of an acid similar to vinegar, and of an additional quantity of carbonic acid and melasses acid.

For in the course of 12 or 18 hours the juice mantles by the rise and escape of carbonic acid in the elastic state: at an earlier period it smells sour or acetous: and by the effect of such delay on the sugar producible from it, it is certain that there is an addition to the original quantity of the melasses acid.

This last is the ingredient which most powerfully impedes the crystallization and separation of the saccharine matter from the deliquescent sweet and mother-liquor called melasses. As it lessens the quantity of saccharine crystals, and increases that of melasses mother-liquor; and as it is highly probable that melasses contain the like acid as a constituent principle, I give it the temporary name of melasses acid.

Herbaceous matter is that of which some part shows itself in the yawing, and more in the boiling of juice which had been cleared from gross filth by filtration. It is that which we endeavour to separate from the saccharine liquor by yawing and skimming.

The herbaceous matter has some analogy to gummy resins, but has a much nearer similitude, in chemical character, to the dregs of refined indigo, or that vegetable sub-

* From Part I. of Dr. Higgins's Observations, &c. published in Jamaica.

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stance which constitutes the chief difference between the finest and the basest indigo.

The herbaceous matter of cane-juice, like that of indigo, varies with the constitution of the plant in different soils and seasons, and especially in respect to its solubility; insomuch that some juices hold about $\frac{1}{300}$dth part of it in strict solution after boiling, while others hold not 1000dth.

But as herbaceous matter is rendered more soluble by the intervention of carbonic acid, any cane-juice holds more herbaceous matter in solution before it has been heated, than it can retain at the temperature of yawing or boiling.

For in the augmented temperature the carbonic acid forsakes the herbaceous matter to combine with that which makes the acid aëriform; the minute gaseous bubbles in their escape agitate and impel the particles lately thrown out of solution, until in their coalescence they become not only visible but large. We may express this change in the clear recent juice by the agency of fire alone, as the workmen do, by saying the liquor breaks.

Fresh cane-juice begins to break when the heat approaches to 140 degrees of Fahrenheit; and the herbaceous matter, which has felt no greater heat, has an olive green colour.

Whether this be exposed to greater heat, or we advert to that which is thrown out during the subsequent reduction of the juice to sugar, the herbaceous matter is found to change colour with the increase of temperature, through gradations of yellow, olive, and brown, increasing in intensity and darkness until the matter is charred to blackness. As it changes in colour it becomes less soluble. The carbonic acid continues to escape, and the extricated herbaceous matter accumulates to the surface, while the liquor is heated to 195°.

Now the watery vapour arising with the carbonic acid bubbles, pushes the cleansed liquor frothing white through intervals in the swollen scum. This, which is called *yawing*, shows that a greater heat would cause a boiling commotion: but if some time be allowed for the residuary carbonic acid to escape, the liquor will not boil until this heat amounts to 206°, or within five degrees of the heat of boiling water.

The aëriform bubbles entangled in the herbaceous matter render it more buoyant than it would otherwise be, and enable it to carry with it, and to sustain at the surface, any accidental filth of the liquor: and the scum thus produced is by its own nature sufficiently tenacious to be separable by the skimmer, or by drawing away the depurated juice from beneath it.

But, if the buoying bubbles be expelled by greater heat and the commotion of boiling, the scum will be broken into the liquor.

The skimmer will now avail nothing; but the herbaceous matter, once thrown out of solution, will subside with the filth, in an hour, in a cooling quiescent liquor, and will leave it transparent, although it still retain that quantity of herbaceous matter, which the water, with the last adherent portions of carbonic acid, can dissolve.

But as this depuration by subsidence cannot be awaited without injury to the juice, the foul scum ought to be removed before the liquor boils.

In consequence of this limited solubility, the residuary herbaceous matter becomes extricated afterwards in quantity proportionate to that of the watery solvent which is expelled by evaporation, and the reduced liquor becomes turbid by the extricated herbaceous particles.

However often the process of evaporation is stopped, and the liquor is depurated to perfect transparency, by subsidence or otherwise, it will become turbid again, by the deposition of herbaceous dregs, when the evaporation is renewed; and it will thus yield dregs to the end, or until the residuary liquor becomes so far saturated with sugar as to be incapable of holding the less soluble herbaceous matter in solution.

All this takes place whether a moderate dose of temper be used or not. But the cleansing by subsidence is quickest when temper is used.

To ascertain the true use of temper, we must advert to its agency on herbaceous matter, carbonic acid, and melasses acid.

Lime powerfully attracts carbonic acid: and although

lime be a soluble body, and although it meet the acids in the aëriform state, it forms with it quickly an insoluble body similar to whiting or chalk.

Lime also combines with a triple or quadruple quantity of herbaceous matter to form a compound less soluble than the latter in water; and in cane-juice, lime meeting carbonic acid and herbaceous matter, unites with both to form a triple compound. For if the lime used in clarifiers were to unite with the carbonic acid only, we should find bottoms consisting of whiting, which I have looked for, but could never obtain.

It is by virtue of these relations that a small quantity of lime, or transparent lime water in which the lime can be only $\frac{1}{500}$th part of the whole, when added to cane-juice that has been duly cleared, renders it presently turbid with herbaceous matter now extricated, and thus facilitates the abstraction of this matter by subsidence. Thus, also, cane-juice which is a little wheyey or clouded is broken to flocculence by transparent lime water as well as by lime.

I say the liquor is broken to flocculence when the particles of herbaceous matter, seized by those of the lime, and coalescing, appear large and flocculent; and the liquor interceding them is seen quite transparent when viewed by transmitted light in the narrow part of a wine-glass.

This breaking may also be distinguished in a bright silver spoonful of the liquor by reflected light.

On these grounds some lime ought to be added to cane-juice which contains the ordinary quantity of herbaceous matter, not with the vain hope of separating all the herbaceous matter at once, but with the experienced certainty that the liquor yawed or cleansed with the aid of lime, will contain less herbaceous matter in solution than it would otherwise have retained, and will require the less additional lime to act on the melasses acid.

Towards the kind of depuration which can be effected in the process of yawing, lime thus contributes something, but not nearly so much as has been generally supposed: for a quantity of lime which is sufficient to give a nauseous taste

to the sugar, is yet incompetent to the extrication of all the herbaceous matter, so that it shall be separable by yawing or subsidence: and an excess of lime, not greater than $\frac{1}{1000}$dth or $\frac{1}{3000}$dth of the weight of the juice, is constantly attended with a manifest debasement of the colour of the sugar, when this excess takes place in the beginning of the boiling, or previous to the reduction of the juice by evaporation. It is of no practical use to inquire after every agency by which the excess of lime has these effects; but it is expedient to observe, that when a juice is yawed with excess of lime, and cleared to transparency by subsidence, which soon takes place in a specimen quickly cooled in a wine-glass, it will show colour approaching to that of porter; while the like juice, treated in the same way, but with only a moderate dose of temper, will be almost colourless when transparent.

It is moreover to be observed in ordinary practice, that when too much temper has been used in the yawing, the liquor, during the boiling of the teaches, looks much browner than that which has been less tempered. The scum has a darker colour, and is more apt to break and sink into the liquor; and it has less of the tenacity and floeculence by which ordinary scum clings on the skimming instrument, and is separable by the common process.

The practical inference from all these facts is, that the temper ought to be used sparingly in the raw juice in the operation of yawing, although it should be found necessary to use more temper afterwards, for purposes different from those lately recited.

It is chiefly by reason of the agency of the temper on the melasses acid, or on that matter which most powerfully impedes the separation of the sugar from the melasses mother-liquor, that the temper is eminently useful in the manufacture of Muscovado sugar, and that greater quantities of it may be advantageously employed, provided the whole of it be not administered at once, and at the period of the manufacture in which it is apt to colour the juice, to lessen the buoyancy and tenacity of the scum, and to frustrate the labour of the skimmer. Among the signs which may guide

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an intelligent sugar-boiler in respect to the doses of temper, those which correspond with the foregoing observations are the simplest and most certain.

When the artist knows that the quantity useful in yawing is less than is necessary for a bold grain and speedy curing of the sugar, and is apprised of the practicability of abstracting all the temper that may be used afterwards in the teaches, he will not err by excess of temper in the first process.

Supposing that he tempers in a grand boiler, or uses a clarifier to the same effect, he will introduce a quantity of temper rather too small than too great, when the vessel has received three-fourths of its charge. He will yaw and check the fire and skim, and then boil moderately and skim for eight or ten minutes. He will then take a wine-glass full of the liquor, and if, after cooling a little, it looks wheyey and not broken, he will use one-sixth or one-fourth of the first quantity of lime, and will immediately take a fresh sample. When the liquor appears broken, and clears with little or no colour, he has hit the precise quantity which is compatible with the cleanness and whiteness of the sugar, and which goes nearest to the whole quantity to be used for the perfection of the grain.

By the usual ladle-proof he will soon ascertain the additional quantity of temper to be used in the inspissated liquor, in the manner presently to be described.

He that will not follow this rule may take a grosser, by using three-fourths of the temper in the yawing, and the remainder in the inspissated liquor.

It has not been unusual for the boilers to put temper in the second and first teach, when the proof by the ladle showed that the temper used in yawing was not sufficient for the granulation of the sugar. Such temper there used was the least injurious to the colour of the sugar; but as none of it could now be separated by skimming from the thick syrup, it manifestly adulterated the sugar, and increased the quantity of filth to be seen in a solution of it.

But no such objection lies against the free use of temper in the syrup, provided we can extract all this temper and all the filth which it catches, and advance the syrup transparent and

and clear, to yield sugar emulating if not equalling ordinary coarse lump sugar, for general use, without abatement of the described celerity in the transition from the raw juice to sugar. For, in a just comparison of sugars to be used œconomically, we are to advert, not merely to the degrees of whiteness, which may be varied by the size of the grain, but chiefly to the cleanness of equal solutions, and the sweetening effects of portions equal in dryness and in weight.

The common use of yellow sugar-candy, on the continent of Europe especially, shows that it is not the colour of good Muscovado sugar which has impeded the consumption of it in the crude state. It is truly the filth which appears in a solution of it, which has forbidden the culinary use of it, and depreciated it so far below lump sugar; and it is in consequence of the cleaning by filtration, rather than by reason of the blanching by breaking the grain and by claying, that the lump sugar has such preference and greater price.

It is this filth, also, which compels the sugar refiner in Europe to use ox blood in quantity proportionate to the impurity of the Muscovado sugar; and to encounter the labour and waste attending the abstraction of the sweets from the abundant scum, consisting of the heterogeneous matter entangled in the filaments of coagulated blood; and it is the chief cause of his inability to give a price for dry Muscovado sugar, nearer to that at which he sells the ordinary lump sugar.

Of the Improvement of Filtration.

It will therefore be the first of all services for this island, and it will be the most desirable character of Jamaica sugar, *that it be made clean by the mere means of lime and filtration*, which are employed in the manufacture of the finest lump sugar; and, until this character is established, little or nothing ought to be said concerning the other tempers and expedients which may be advantageously used in particular circumstances.

As the true meaning of the word *filtration* is not understood by all, and doubts and controversies may arise from the mere abuse or misconception of the term, it is expedient that it should be defined in a manner suitable to all the workmen,

workmen, to whom these pages may ultimately be communicated.

When a foul liquor is passed through a fine wire sieve, or through a blanket, which stops the coarse filth, but not the finer, and where the liquor so treated still remains clouded or turbid, this is called *straining:* but when a medium, such as close cloth or unsized paper, is used, and this stops all extricated filth and passes the liquor transparent, the process is called *filtration.*

In the refineries in Europe a blanket serves as a filter, but not before the filth of the solution of Muscovado sugar has been entangled in flocks of coagulated ox blood, too large to pass through the interstices of a blanket.

The use of such a woollen strainer has been tried in this island, but is generally abandoned for good reasons.

A blanket only strains the boiled cane-juice, while it passes with the velocity necessary for the manufacture of good Muscovado sugar. When the interstices of the blanket are narrowed by coarse flocculent filth, and when it begins to filtrate truly, the process becomes so slow as to retard the manufacture intolerably: the last portions refuse to pass through; the quantity of sweets retained in and on a large thick blanket makes a great defalcation from the sugar, and the cleansing of the cloth is laborious.

To make the process of filtration applicable in the manufacture of Muscovado sugar, it was necessary that a medium should be employed capable of filtrating truly at the beginning. It was equally necessary that the velocity of this filtration should be equal to that with which the liquor would pass turbid through a coarse strainer of the same dimensions. It was moreover requisite, for the reasons above mentioned, that this rapid filtration should be confined to that period in which all the herbaceous matter is thrown out of solution, and in which the liquor is viscid with sugar.

These desiderata are attained in the invention lately presented under trial to as many of the honourable members of the appointed committees as could be assembled at the time and place.

In this first mechanism the proof of the principles and powers

powers was chiefly attended to: in the instruments which are now to be made, the utmost convenience will be attained; and from these only the descriptions and models, which I shall soon lay before the honourable members, are to be made.

At the present moment I must content myself with presenting a few lines descriptive of the principles upon which a small simple filtrating instrument is competent to the intended uses.

In ordinary cases, the power by which the liquor is pressed through the interstices of a filtrating cloth is merely the weight of the liquor; and, under equal pressures by weight, the velocity of filtration is as the extent of filtrating cloth. But, as in ordinary vessels used for this purpose, the pressure on a given area of the filtrating surface increases with the height of the liquor above it, omitting nice discussions, we may affirm generally, that the velocity of filtration is as the number of square inches of filtrating cloth multiplied by the height of the contained pressing fluid; therefore, in order to attain any required velocity of filtration, the height of the pressing column may be made to compensate for any desirable reduction of the size of the cloth.

Supposing, for instance, a cloth measuring 8 feet by $7\frac{1}{2}$, or 60 square feet in area, could filter fast enough under a charge of liquor measuring 3 inches in depth, one-sixth of this cloth, or 10 square feet of it, would serve if the column or charge of liquor pressing on it were made to be 18 inches in height: and with the height of 36 inches, 5 square feet would serve; and this is less than the measure of a common pocket handkerchief.

By the like reasoning it will be found, that if the cloth of 60 square feet filter no quicker than is necessary for the progress of the manufacture, the filtration will become slower as the liquor lowers to a smaller area of filtrating surface, and still slower as the height of the pressing liquor decreases; so that at the height of one inch, the quantity filtrated in a given time would be much less than one-third of that which the progress of the work would require.

But when it is considered that filtration will not avail much

much until the liquor is rich and viscid with sugar; that every filtration abates as the interstices of the cloth become narrowed, or clogged with filth; and that a rich saccharine liquor expanded on a large cloth in a basket, in the usual mode of filtration, will soon be chilled, and incapable of passing through the chilled and agglutinated filth; it will readily appear that the quantity of sweets retained in and upon the cloth would cause an enormous defalcation of the sugar, and great labour in washing out the sweets for the stillhouse, and the filth from any great filter. We are to revert, then, to the small filter above mentioned, acting by the pressure of a high column of liquor, in order to obviate the described inconveniences.

It may appear paradoxical at the first view, but it is a fact demonstrated in every school-book of natural philosophy, that the pressure of the liquor on the filter is the same whether the superincumbent column consists of a ton or of a pint of the liquor, provided the heights of these columns be equal.

Therefore, instead of making an instrument to give a ton of pressure, by containing about a ton, or 250 gallons of liquor, I have given an equivalent pressure by a pint or two contained in a slender perpendicular tube: and instead of setting the filtrating bag to a great capacity, I have set it vertical, flat, and with the opposite parallel sides so near to each other, as to leave room only for the filth to be collected from half a ton or a ton of sugar.

By such mechanism and pressure the juice may be filtrated truly and expeditiously, even when it is so rich and viscid with sugar that it could not pass through any filtrating medium used in the ordinary way. The filtration keeps pace with the ladling forward, when the cloth measures about 10 or 12 feet square, and the flattened bag about 5 square feet: and the same bag will clean from 10 to 15 or 20 hundred weight of sugar, according to the nature of the juice and the care of the workman in the previous depurations by yawing and skimming.

By these means, also, the whole quantity of liquor in the filter at any time may fall short of a gallon; and the quantity

tity retained when the bag becomes stuffed and clogged with filth is too inconsiderable to deserve notice, even if the washings were not to go to the still-house. For the described pressure of the slender column of the liquor is, or may be, made competent to squeeze the filth with a force equal to that of one, two, or three tons weight, and to reduce it to the consistency of dough, if that were necessary.

The cloth which answers for this purpose by its closeness, and durability, and cheapness, is a kind of flannel called double swanskin: it is sewed to form a bag: the bag is placed flat between two reeded or fluted faces of wood: the mouth of the bag is perfectly closed, by giving it one fold or fell to be compressed by the reeded faces. The liquor is supplied from the slender pressing column by a small stop-cock entering a small short tube of swanskin belonging to the bag: by drawing a little of this tube over its orifice, the ligature becomes closer as the pressure of the liquor becomes greater.

It is easy to conceive how quickly such a filter may be slung into its place or removed for washing, or to make way for another that is already washed.

To prevent fruitless experiments and expenses it is necessary to observe, that in any position different from the vertical now described, the bag will not act as a filter, and will only strain the liquor, and that, if the reeding be not vertical, the filtration will be slow.

When liquor is to be thrown forward, it is, in the general course of business, to be ladled from the second teach into the instrument, by the gutter, off which it will run clear and quickly into the first teach.

When the ladle is held in the usual method, the labour of throwing the liquor to the described height would be considerable: but it becomes uncommonly easy when the hook of a slender pendulous chain or rope is made to sling the handle of the ladle at a spur, marking the proper centre of motion, or the fulcrum on which the ladle is to play, with little effort to the workman.

At skipping-time the instrument reserves some filtered syrup to recruit the emptied teach: this is the business of a few

few minutes: if it were as many hours, such syrup would take no damage in the time.

About twelve ladlings forward belong to a skip; and for each of these its share of reserved temper is to be used. Less than a tea-spoonful generally serves: a small excess does no harm now to the colour of the sugar: both the filth and the temper are stopped in the filter; and the sugar becomes improved in the manner above described, provided no part of it be over-heated in the skipping.

The damage which the liquid sugar takes here is certainly greater, as it is fouler by the herbaceous matter; because the herbaceous matter becomes darkened or charred sooner than the mere sugar. But whether the filter be used or not, there will always be some depravation of the colour of the ground sugar when the skipping is conducted in the usual manner.

Of the Spraying Instrument in Skipping.

It is now sufficiently understood, that in boiling down and in ladling forward, great care ought to be taken that no part of a vessel, between the surface of the charge and the under-pinning, shall be so far heated as to burn-to the syrup or sugar adhering to it; and that this burning or charring is to be prevented by one negro rolling the residuary liquor up to the under-pinning, until another has ladled forward and charged the evacuated teach, and until the liquor froths to the under-pinning.

But as nothing of this kind is practicable in the skipping from the first teach, some new expedient is necessary to prevent the burning-to which is manifested in every whole skip, by the hissing heat of the copper, by the empyreumatic smell, and by the film of charred matter which washes from the sides into the subsequent charge of liquor; for the practice of some, in skipping only one-half of the ready and graining charge, is a very objectionable shift.

To preserve the crop and grain once attained, to prevent all empyreuma, and to maintain every advantage of the filtration, I have contrived, and presented in trial to the honourable

nourable gentlemen of the committee, a simple instrument, which the negroes have since used with the greatest ease.

It is nothing more than a tube of tinned iron or sheet copper, about six feet and a half in length, with a bore of one-half or three-fourths of an inch in diameter, delivering a spray of water on the bottom and sides of the skipping-teach during the short time of skipping, and cooling it sufficiently and equally with a very small quantity of water.

Upon the common shed or arched covering which shelters the stoker, a cask of water is placed, and a slender hose or leathern tube inserted in the cask, delivers the high column of water into the metallic tube through a small stop-cock; and the other end of the metallic tube being provided with six small holes, like those in the top of a pepper-box, and looking upwards when the tube is held horizontal, the water is squirted in the form of spray by the heavy pressure of the high column to the extent of the bottom and sides of the teach only, and the spray ceases when the cock is closed.

The direction of the spray requires no skill of the negro; for a spur on the tinned iron tube stops it at the proper place; and he has only to hold the tube straight forward, while he grasps it near the spur and near the mouth-piece of the fire-place with one hand, and holds or turns the cock with the other.

The fire is not disturbed, and no wet trash is allowed to chill the furnace and clot upon the grating. The spray evaporates as it touches and cools the teach; and as the fire-face of the furnace remains untouched and red hot, the fresh charge boils in the first teach in half a minute after the spray is stopped, and the instrument now shut at the cock is withdrawn.

The advantage of this instrument is not confined to the prevention of empyreuma and colour; for it serves to make all the portions of the skipped mass equal in spissitude, or, as the workmen say, equally boiled, and enables the workman to defer the skipping until the proof is perfectly decisive; and these purposes are answered at the expenditure of two or three quarts of water.

Subservience

Subservience of these Measures to the Improvement of Rum.

Concerning rum it is now to be observed, that it derives the depreciating characters of the recent spirit from two sources; the chief of which is the filth of the scums, and especially the first scums in yawing.

The tendency of such matter, even if there were nothing verminous or animalcular in it, is to the putrefactive fermentation, or rotting, while that of the sweet is to the vinous fermentation, and thence to the acetous; the product of the former fermentation is as offensive to the smell and taste, and as noxious, as that of the latter is grateful and cordial. Wherever scums are detained to await the spontaneous separation of the sweets from the filth, an intestine motion may be observed, and then chiefly in the concurrence of these fermentations the offensive product is generated; the rest is formed in the fermenting vats, in quantity proportionate to the filth of this kind which passes into them.

Every vinous liquor capable of yielding an intoxicating spirit by distillation, affords some quantity of peculiar essential oil, which awaits the arise of the water of the latter and weaker runnings, and characterizes them; therefore this essential oil is, in a great measure, separable from the spirit by redistillation; especially if salts retentive of the water, and restraining the volatility of the oil, are used.

But it is peculiar to the ordinary manufacture of rum that very offensive ethereal fluid is generated in these mixed fermentations, and that by reason of its volatility it is inseparable by a redistillation.

But from the source above mentioned the essential oil of rum acquires extraordinary nauseousness; and as a single redistillation cannot exclude it totally, and as any number could not exclude the ethereal taint above mentioned, the best new rum of any estate is that which runs intermediate in respect of the offensive ether and the fetid oily faints.

All rum is improved by time in wooden casks, by exhalation of ether and absorption of oil, and under a growing charge for waste and for interest on the price. Some have improved it sooner by ventilation, but not without a great

waste of spirits; but now it may be remarkably improved immediately by measures which prevent the above contamination; and the first of these is the abstraction of the putrefactive matter by filtration, and the immediate conveyance of the clear warm fragrant liquor to the working cistern, there to undergo the most timely and productive fermentation, and to suffer the least defalcation of spirit by foul scum and bottoms, which are generally thrown away.

Another source of the contamination is in the empyreuma; but as this regards the distillation as well as the errors in making sugar, it is unnecessary to say more of it at present than that the prescribed measures, together with a judicious setting and management of the still, will totally prevent the empyreumatic smell and taste.

LVII. *Letter of M.* Tardy de La Brossy *to Professor* Pictet, *of Geneva, upon the Experiments of Mr.* Biddle, *relative to the Density of frozen Mercury**.

Joyeuse (Ardèche),
Oct. 13, 1805.

Sir,

The experiments of Mr. Biddle (vol. xxxix. p. 217†), in order to determine the density of solid mercury, having attracted my attention, I cannot pass over in silence the serious objections to which the results of the above gentleman are liable. In communicating them to you, I am well persuaded that I shall enter completely into your views, which have equally for their objects the extension of truth and the removal of error.

Mr. Biddle, after having made known the process which he followed and the precautions taken by him, informs us, that a thousand grains of mercury made solid at the 40th degree below the zero of Fahrenheit (— 32 of Reaumur), experienced, when weighed in alcohol of the same temperature, a loss in weight of 59·8 grains.

A thousand grains of pure silver, weighed by the same

* From *Bibliotheque Britannique*, vol. xxx.

† *Vide* Nicholson's Journal for April 1805.

balance,

balance, in the same alcohol, and at the same temperature, lost 88·105 grains of their weight. Upon this, Mr. Biddle, regarding the loss of weight of the mercury and silver as in an inverse ratio to the specific weights of these metallic substances, and having found that that of silver, with the same balance and in distilled water, was 10·436, he multiplied this sum by 88·105, and divided the product by 59·8, which gave him 15·612 for the specific weight of the mercury in a solid state.

But the same hydrostatic balance having given the number 13·545 for the specific gravity of the same mercury in the liquid state, the thermometer being at + 47° Fahr. (+ 6½° Reau.), it would seem that nothing remains but to conclude with the author, "*that the volume of solid mercury is less by about one-seventh of what it is in the liquid state.*"

But are all the principles of these calculations faultless? Are none of these weights evidently wrong? I shall not make a gratuitous supposition; I shall judge by facts alone.

The antient tables of specific gravities give 0·806 for that of alcohol or rectified spirit of wine. It is at about 0·820, in the temperature of 19° of Reaumur (+ 51½° Fahr.), according to M. Bories, of Marseilles, at which the operations on these substances can be regarded as decisive authority. We then fairly value it 0·810, without encroaching much upon truth. But the liquid mercury, which, according to its specific gravity of 13·545, loses in distilled water 73·828 grains in the thousand, ought to lose 59·8 in alcohol at the weight of 0·810; and in this manner the great difference disappears which Mr. Biddle thought himself entitled to mark between the density of liquid and solid mercury.

Pure silver, which in consequence of a specific gravity of 10·436 ought to lose in distilled water 95·822 grains in the 1000, would lose no more than 77·615 in alcohol at 0·810, which is very different from 88·105. To account for this difference, we cannot allege the greater density of the alcohol at the temperature of — 47° Fahr., because that could not contribute *more than two or three grains at most* to the loss of the weight of the silver: we should not be better founded in supposing that the alcohol employed happening

to be of an inferior quality, its specific gravity was greater, because upon this supposition, and in order to make the loss of weight in the silver amount to 88·105 grains, the specific gravity of this alcohol, compared with that of water, must have been 0·920. But Mr. Biddle could not but be mistaken with such a liquor, which was nothing more than the most common spirits, and which, if I am not mistaken, could not support the temperature of the experiment without freezing.

It appears, therefore, ascertained, that there is an error in the weights; and consequently we may conceive that, the density of the solid mercury having been deduced from the comparison of the results of these weights, the density has been found so much the greater as the loss of the weight in silver had been expressed by too large a number.

I have no intention to pre-judge the question relative to the density of solid mercury: I am far from wishing to raise doubts upon the talents of Mr. Biddle, or upon the penetration of the members of the Philosophical Society of Birmingham, in whose presence and to whose satisfaction the experiments of the former were made; but I think myself entitled to say, that the experiments, of which he has published the results, ought at least to be made over again. Mr. Biddle's idea is a happy one, and I do not deny that these experiments may lead to the solution of the problem.

I have the honour to be, &c.

TARDY DE LA BROSSY.

LVIII. *Analysis of the sulphuretted Oxide of Manganese of Naygag. By M.* VAUQUELIN*.

M. KLAPROTH, having made an analysis of the sulphuret of manganese under the name of *siebenburghischen*, discovered that it is composed of 82 parts of oxide of manganese at the minimum, or soluble in nitric acid; of five parts of carbonic acid, and of eleven parts of sulphur, without

* From *Annales du Muséum d'Histoire Naturelle*, vol. vi.

having

having discovered either gold or silver in it, as asserted by M. Muller, of Reichenstein.

The celebrated Berlin chemist having thus enriched mineralogy with a new species of the manganese genus, which down to the present time had existed only in one form, that of oxidated manganese, he has also thereby enriched chemistry with a new fact concerning the action of the nitric acid upon sulphurets, for the decomposition of which it is usually employed, with the view of dissolving the metallic oxide combined with sulphur, without touching the latter.

As soon as M. Haüy recognized this mineral in his collection, he sent us a specimen in order that we might submit it to analysis.

M. Proust also analysed this mineral almost at the same moment with M. Klaproth. The former observed the same phænomena, but for want of a large enough quantity he was not able to determine the state in which manganese is when combined with sulphur, nor the respective quantities of these bodies.

The sulphuret of manganese of naygag is accompanied with manganeseous carbonated lime: it has for a matrix a white hyaline quartz: its specific gravity is 4: its texture is lamellous, with a metallic lustre, when the surface has not been long exposed to the air. Reduced into powder, it is of an olive green colour: it loses nothing by heat.

Five grammes of this mineral, perfectly freed from its matrix, were reduced to a very fine powder and treated with weak nitric acid; which immediately exercised a lively action upon it, accompanied with a disengagement of sulphuretted hydrogen gas. We gathered a certain quantity of this gas, in order to examine its nature. The mixture was slightly heated, and a new portion of the nitric acid was introduced until effervescence ceased: the liquor was then filtered, which was a little reddish, but became colourless upon the addition of water. The residue weighed a decigramme. It was composed of blackish brown flakes, which, upon being exposed to flame with the blowpipe, took fire like sulphur, spread a slight arsenical smell, and left a substance which did not colour borax like manganese, but like iron.

The gas obtained in this experiment, when passed through lime water, did not injure its transparency, but gave it the property of blackening solutions of lead.

The nitric solution was mixed with carbonate of potash; it formed an abundant white precipitate with a brisk disengagement of carbonic acid gas. We heated it lightly in order to drive off the excess of this acid; and we separated the precipitate, which, upon being well washed and dried, weighed $7\frac{1}{2}$ grammes. The difference between our results and those of M. Klaproth seems to arise from the manganese used by us having been very pure, while that employed by M. Klaproth probably contained carbonate of lime.

M. Klaproth endeavoured to form a combination between sulphur and the oxide of manganese at the minimum, in order to make comparative experiments, and he discovered that the artificial sulphuret of manganese, when no atom of sulphuretted hydrogen could enter into it, had the same characters as the natural sulphuret. In order to follow his experiments, I calcined in a retort, the aperture of which communicated with a balloon filled with lime water, 7·4 grammes of carbonate of manganese obtained by precipitation by means of carbonate of potash.

The carbonic acid gas began to disengage itself before the retort was red, and at the end of a quarter of an hour's calcination the disengagement ceased. The oxide contained in the retort was slightly coloured, at least at its surface. We introduced into the retort, while yet warm, two grammes of flowers of sulphur, and we agitated it in order to produce a mixture; the mass melted, and a considerable quantity of sulphur was sublimed. As soon as the sublimation of the sulphur ceased, the mass, when taken out of the retort while warm, took fire on exposure to the air in the manner of pyrophorus. It was green, like the natural sulphuret in powder; and it weighed 5·9 grammes. This artificial sulphuret dissolved in weak nitric acid with effervescence and a disengagement of sulphuretted hydrogen gas; but it leaves more sulphur as a residue than natural sulphuret.

The following is the manner in which M. Klaproth explains the disengagement of the hydrogen gas which takes place

place during the solution of manganese in the nitric acid. In spite of the disengagement, says he, of a considerable enough quantity of sulphuretted hydrogen gas during the solution of this mineral, it appears to me that it would be an error to believe that this gas exists ready formed in the mineral or in any of those which yield it by the humid way, and to regard it as one of their constituent parts. There is no doubt that it is formed by the decomposition of water, since by calcination we obtain nothing else but carbonic acid gas. By synthesis, the probability that no hydrogen enters into the combination of sulphur with the oxide of manganese acquires still more force; and yet this combination yields sulphuretted hydrogen gas with the acids.

In order to know if the nitric acid is not decomposed during the solution of sulphuret of manganese, as happens with almost all the metals which have a great affinity for oxygen, or, if water alone, by yielding its oxygen to one of the elements of this mineral, does not give birth to this hydrogen gas, we dissolved a certain quantity of sulphuretted manganese in weak nitric acid; we concentrated the solution, and distilled it in a retort with caustic potash: but the produce not having given any sign of the presence of ammonia, we concluded from this that the nitric acid is not decomposed in this operation. In order to have the just quantity of oxide of manganese at the minimum, we calcined in a retort 7·4 grammes of carbonate of this metal prepared from a solution of five grammes, and we obtained an oxide almost white, which, weighed while warm, yielded 4·25 grammes, which was at the rate of 85 in the hundred.

Let us actually admit a loss of two parts: we shall then have 13 parts of sulphur; and as the loss can scarcely be any thing else than sulphur, the quantities will stand thus:

Manganese at the minimum -	85
Sulphur - - - -	15
	100*

* There is also in this mineral a small quantity of iron and arsenic, which has been discovered among the sulphur which remains after its solution in weak nitric acid; but these substances appear to be accidental.

Reflections.

M. Klaproth justly regards the manganese in the mineral of which we are speaking as oxidated at the minimum, and he has recourse at the same time to the decomposition of water in order to explain the disengagement of sulphuretted hydrogen gas which takes place during the solution of the mineral in the acids; but this philosopher neither tells us how or wherefore water is decomposed in this operation. The manganese cannot be the cause of it, since it is already united to oxygen, and because it is discovered in the acids which have dissolved it in the same state in which it existed in the fossil, that is to say, at the minimum. The water, then, could not have been decomposed but by the sulphur. But how can we comprehend this effect while the nitric acid is present? If, however, it is the sulphur which decomposes the water, and which gives birth to the sulphuretted hydrogen, I should have found sulphuric acid in the nitric solution of the mineral. In order to ascertain it, I dissolved in the cold a certain quantity of the same sulphuret of manganese in weak nitric acid, in order that it might not burn the sulphur. The phænomena were the same as before; and the filtered solution gave, in fact, by means of the muriate of barytes, a precipitate which was a true sulphate of that base.

This experiment, then, seemed to demonstrate, that sulphur united to oxide of manganese has the power of decomposing water by combining with its oxygen, and thus sets its hydrogen at liberty, which unites with another portion of sulphur. This fact is the more worthy of the attention of chemists, that, to my knowledge, this is the first time that it has been observed, and that in every case where metallic sulphurets or sulphuretted oxides have been decomposed by the strong or weak nitric acid, it has been always the latter which has been decomposed, and nitrous gas, or modifications of it, constantly obtained, and never sulphuretted hydrogen gas: this is quite conformable to the laws of chemical affinity. It is true, that there are metals which decompose water at the same time with nitric acid; but hydrogen never makes its appearance: it unites with the azote of the nitric acid and forms ammonia.

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The sulphuretted oxide of manganese thus forms an exception to the rule hitherto observed, if we do not admit hydrogen into this substance as one of its constituent parts.

LIX. *Memoir on the Eremophilus and Astroblepus, two new Genera of the Order of Apodes. By M.* De Humboldt *.

When we ascend the chain of the Andes to the height of 2600 toises (16661¾ English feet) and upwards, great level plains and lakes of a considerable extent are seen. It is singular to observe, that, while the soil is still covered with a beautiful vegetation, the woods filled with quadrupeds, and the air with a great variety of birds, the water alone, the lakes and the rivers, are so little inhabited. The cause of this phænomenon relates, without doubt, to geological facts; it pertains to the grand mystery of the origin and migration of species.

The considerable lakes which surround the city of Mexico, at the height of 1160 toises †, nourish but two species of fish, of which one, the *axalotl*, belongs rather to the genera *sirenus* and *proteus*. M. Cuvier, to whom we brought this extraordinarily organized animal, unknown in Europe, is engaged with its anatomy, which he will shortly publish. In the kingdom of New Granada, in the beautiful valley of Bogota, about 1347 toises high, there also exist but two species, which the inhabitants of that country call *capitan* and *guapucha*. The one is an *atherine*, and the other a new genus of *apodes*, that I am about to describe in this memoir. The form of its tail and its anal fin distinguish it sufficiently from the genus *trichiurus*, which is also found in the fresh waters of South America. I have designed this non-descript fish at the place; and Messrs. Lacepede and Cuvier, who have willingly examined my descriptions, like me, consider it a new genus well characterized. I have named it *eremophilus* on accout of the solitude in which it lives at so great

* From *Recueil d'Observations de Zoologie et d'Anatomie comparé*, 1re livraison. Communicated by a correspondent.

† The French toise is about six feet four inches nine-tenths English.

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an elevation, and in waters which are inhabited by almost no other living being. The naturalists, who fear that new species of the same genus may be discovered in very different situations, may change the name of *eremophilus* into that of *thrichomycterus*, taken from the barbillons or whiskers attached to the nose of this fish.

EREMOPHILUS. (See Plate VIII.)

APOD. CHARACTER GENERICUS ESSENTIALIS.

Corpus elongatum. Cirri maxillares 4, *nasales semitubulosi* 2. *Pinna dorsalis et analis. Membrana branchiostega radiis* 1—2.

E. MUTISII.

Corpore elongato, plumbeo, cærulescenti, maculis dædaleis olivaceis variegato; operculi branchiostegi; duplicatura spinuloso-serrata.

The body of the *captain* of Bogota is long, and has some analogy with that of the eel. It is compressed, of a blueish gray colour, and spotted with olive green. These spots, the outline of which forms very striking sinuosities, assume in some individuals a yellowish tint. The head is little and flat. The mouth, situated at the extremity of the nose, is straight. The upper jaw projects over the under one: the first, very long and double, is furnished with six fleshy barbillons or whiskers, of which the two exterior are the longest. Two other barbillons, shorter and semi-tubulous, are placed on the nostrils. It has very little eyes, which are veiled by a semi-transparent membrane, like the gymnotes and lampreys. The extremity of its lips is furnished with little teeth resembling hairs. The tongue is very fleshy, but short. The operculum or uvula forms a very narrow branchial opening, and it is very difficult to distinguish its folds (lames). In the most part of the individuals which I have examined, it appears to me that the captain, similar to the *cyclopterus dentex* and a few other fishes, has but two radii or furrows, which are as if soldered the one on the other. The edge of the operculum or uvula is indented. The dorsal fin has eight radii, the pectoral six, that of the anus six, and that of the tail, which is round, twelve radii:

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it has no swim or air-bladder. The length of this fish is from 10 to 11 inches, and its body is covered with a mucus common to the greater part of the apodes. It inhabits the little river of Bogota, that forms the famous cataract of Tequendama. The captain is a very agreeable aliment, and so much the more precious, that without it the inhabitants of the capital of Santa Fé, in the time of Lent, would be reduced to the use of only salted sea-fish brought from a great distance. I have given this species the trivial name *Mutisii* in honour of the celebrated naturalist, whose rich collections are preserved in the great valley of Bogota.

The little river of Palacé, near Popayan, nourishes another fish, which, by its mucosity and the position of its fins, has some relation with the *eremophilus*, but which ought also to constitute a new genus of apodes. The breadth of its head is greater than that of the body; its eyes placed on the upper part of the head, and turned so that the pupils are directed, like as in the *uranoscopus mus*, towards the surface of the water; the indenting of the first radii of the fins, the branchial membrane of four radii; the tongue; the want of barbillons or whiskers on the nostrils; and the dorsal fin, which approaches more to the head than the tail; sufficiently distinguish the *pescado negro* (black fish) of Popayan from the *capitan* of Santa Fé. I have given the name of *astroblepus* to this genus, in allusion to the extraordinary situation of its eyes.

ASTROBLEPUS. (See Plate VIII.)

Apod. Character Genericus Essentialis.

Corpus plagioplateum. Membrana branchiostega radiis 4. Oculi verticales. Cirri 2 maxillares, nasales nulli.

A. Grixalvii.

Corpore ex olivaceo nigrescenti, capite subtruncato, radiis pinnarum exterioribus serratis.

Corpus plagioplateum, oblongum, nudum, olivaceo-nigrescens, caudam versus angustatum, subcompressum. Caput obtusum, magnum, subtruncatum. Cirri 2, apice recurvi et sublati, ricto in labio superiori adnati. Maxilla labiata, labio superiori majori plicatili. Lingua nulla. Nares 2 magnæ,

magnæ, margine membranaceo. Oculi verticales, minuti. Operculum simplex, convexum, nudum. Membrana branchiostega radiis 4, ossiculo anteriori subserrato. Pinna pectoralis radiis 10. Pinna analis radiis 7. Pinna caudalis integra radiis 12; radiis duobus exterioribus (ut omnium pinnarum) extrorsum serratis. Longitudo 14 pollicaris.

I have given this fish the specific name *Grixalvii* * to perpetuate the memory of a respectable philosopher, Don Mariano Grixalva, who has disseminated at Popayan a taste for the physical sciences, which he himself cultivated with success.

The *pescado negro*, so much eaten at Popayan, is found but in that part of the river Cauca which is most contiguous to the city. The physical cause of this phænomenon is sufficiently striking. From the volcano of Purasé descends a rivulet impregnated with sulphuric acid, that the inhabitants call *Rio Vinagre* (Vinegar River); it is known by the beautiful cascade which it forms at the foot of the volcano. From the point where the waters of Vinegar River unite with those of Cauca until four leagues lower down the latter is without fish, although in the upper part they are plentiful enough. The small quantities of acid that might escape our chemical analysis are often sufficiently great to injure the organization of fishes.

* *Mutisii* and *Grixalvii* are doubtless very scientific names. Linnæus, to gratify his puerile vanity, introduced the custom of giving arbitrary unmeaning names of men to plants: Werner embraced the same unphilosophical system of pitiable ambition in baptizing minerals (some wits have asserted, indeed, that such is his attachment to water, that he actually performed the ceremony of sprinkling certain stones, giving them at the same time the favourite name of some of his followers): and M. Humboldt now transfers men's names to the very opposite abodes of fire and water, in his volcanic fish! All the labours of these men have done much less to disseminate a taste for the natural sciences, than the introduction of such an absurd practice has effected in obstructing the advancement of real knowledge and true philosophy. Posterity, so far from venerating such names, will execrate the being, who, to conceal his real ignorance by the assumption of universal knowledge, could thus deliberately bury true science and much accurate knowledge under the ruins of a Babylonish jargon! Peace to the manes of Lavoisier: although he himself made no real discoveries, yet the philosophical use which he made of those of the English and other philosophers will not speedily be forgotten by succeeding generations.—*Translator.*

LX. *Me-*

LX. *Memoir on a new Species of Pimelodus thrown out of the Volcanoes in the Kingdom of Quito; with some Particulars respecting the Volcanoes of the Andes.* By *M.* De Humboldt *.

The chain of the Andes, from the Straits of Magellan to the northern shores bordering on Asia, extending over more than 2000 leagues, presents above fifty volcanoes still active, of which the phænomena are as various as their height and local situation. A small number of the least elevated of these volcanoes throw out running lava. I have seen, at the volcano of Zurullo, in Mexico, a basaltic cone that sprung from the earth the 15th September 1759, and at present rising 249 toises (1595⅓ feet) above the surrounding plain. The volcanic ridges of Guatimala cast out a prodigious quantity of muriate of ammonia. Those of Popayan and the high plain of Pasto present either *solfatares*, which exhale sulphureous acid, or little craters filled with boiling water, and disengaging sulphurated hydrogen, which decomposes by contact with the oxygen of the atmosphere. The volcanoes of the kingdom of Quito throw out pumice-stone, basaltes †, and scorified porphyries; and vomit enormous quantities of water, carburetted argil, and muddy matter, which spreads fertility from eight to ten leagues around. But, since the period to which the traditions of the natives ascend, they have never produced great masses of running melted lava. The height of these colossal mountains, that surpasses five times that of Vesuvius, and their inland situation, are, without doubt, the principal causes of these anomalies. The subterranean noise of Cotopaxi, at the time of its great explosions, extends to distances equal to that from Vesuvius to Dijon. But, notwithstanding this inten-

* From *Recueil d'Observations de Zoologie et d'Anatomie comparé*, 1re livraison.

† It would have been of some use to geolog. had the author here mentioned whether the stone which he calls basaltes has been submitted to the action of fire or water; or whether, in addition to the other well known characters of this mineral, it yielded hydrogen gas on distillation, the latter being the peculiar characteristic of what is properly denominated basaltes.—*Translator.*

sity

sity of force, it is known, that if the volcanic fire was at a great depth, the melted lava could neither raise itself to the edge of the crater, nor pierce the flank of these mountains, which to the height of 1400 toises (8971½ feet) are fortified by high surrounding plains. It appears, therefore, natural, that volcanoes so elevated should discharge from their mouth but isolated stones, volcanic cinders or ashes, flames, boiling water, and, above all, this carburetted argil impregnated with sulphur, that is called *moya* * in the language of the country.

The mountains of the kingdom of Quito occasionally offer another spectacle, less alarming, but not less curious to the naturalist. The great explosions are periodical, and somewhat rare. Cotopaxi, Tungurahua, and Sangay, sometimes do not present one in twenty or thirty years. But during such intervals even these volcanoes will discharge enormous quantities of argillaceous mud; and, what is more extraordinary, an innumerable quantity of fish. By accident, none of these volcanic inundations took place the year that I passed the Andes of Quito; but the fish vomited from the volcanoes is a phænomenon so common, and so generally known by all the inhabitants of that country, that there cannot remain the least doubt of its authenticity. As there are in these regions several very well informed persons, who have successfully devoted themselves to the physical sciences, I have had an opportunity of procuring exact information (*renseignemens*) respecting these fishes. M. de Larrea, at Quito, well versed in the study of chemistry, who has formed a cabinet of the minerals of his country, has been, above all others, the most useful to me in these researches. Examining the archives of several little towns in the neighbourhood of Cotopaxi, in order to extract the epochs of the great earthquakes, that fortunately have been preserved with care, I there found some notes on the fish ejected from the vol-

* M. Humboldt seems not to have been aware that this name has been affixed to it in consequence of its having some resemblance to a kind of blackish coarse bread made of grits or pollard, and used in Spain by some very poor but proud people, or for purposes of penitence in cases of a *pecado mortal*.—*Translator.*

canoes.

canoes. On the estates of the marquis of Selvalegre the Cotopaxi had thrown a quantity so great, that their putrefaction spread a fetid odour around. In 1691 the almost extinguished volcano of Imbaburu threw out thousands on the fields in the environs of the city of Ibarra. The putrid fevers which commenced at that period were attributed to the miasma which exhaled from these fish, heaped on the surface of the earth and exposed to the rays of the sun. The last time that Imbaburu ejected fish was on the 19th of June 1698, when the volcano of Cargueirazo sunk, and thousands of these animals enveloped in argillaceous mud were thrown over the crumbling borders.

The Cotopaxi and Tungurahua throw out fish, sometimes by the crater which is at the top of these mountains, sometimes by lateral vents, but constantly at 2500 or 2600 toises above the level of the sea: the adjacent plains being 1300 toises high, one may conclude that these animals issue from a point which is 1300 toises more elevated than the plains on which they are thrown. Some Indians have assured me, that the fish vomited by the volcanoes were sometimes still living in descending along the flank of the mountain: but this fact does not appear to me sufficiently proved: certain it is, that among the thousands of dead fish that in a few hours are seen descending from Cotopaxi with great bodies of cold fresh water, there are very few that are so much disfigured that one can believe them to have been exposed to the action of a strong heat. This fact becomes still more striking when we consider the soft flesh of these animals, and the thick smoke which the volcano exhales during the eruption. It appeared to me of very great importance to descriptive natural history to verify sufficiently the nature of these animals. All the inhabitants agree that they are identical with those which are found in the rivulets at the foot of these volcanoes, and called *preñadillas**: they are even the only species of fish that is discovered at the height of above 1400 toises

* This word is an indifferent or contemptuous diminutive, indicating abundant, pregnant, fruitful, easily taken, but not a pleasing or desirable object. The name is purely Spanish and not Indian, of course could never have been applied to any fish used as food by Spaniards.—*Translator.*

in

in the waters of the kingdom of Quito. I have designed it, with care, on the spot, and my design has been coloured by M. Turpin. I have observed that the *prennadilla* is a new species of the genus *silurus*. M. Lacepede, who has also examined it, advised me to place it in that division of *silurus* which, in the fifth volume of his Natural History of Fishes, he has described under the name of *pimelodes*.

This new species of *pimelodus* has a depressed body of an olive colour mixed with little black spots. The mouth, which is at the extremity of the nose, is very large, and furnished with two barbillons or whiskers attached to the jaws. The nostrils are tubulous; the eyes are very small, and placed towards the middle of the head. The skin of the body and the tail is covered with an abundant mucus, and the mouth is furnished with very small teeth. The branchial membrane has four radii, like the *pimelodus chilensis*; the pectoral fin has nine; the ventral five; the first dorsal six; the fin of the anus seven; and that of the tail, which is bifid, has twelve radii. The first radius of all the fins is indented on the outside: the second dorsal fin is adipose, and placed near the tail. This little *pimelodus*, which is found in lakes even to the height of 1700 toises, is, without doubt, the fish that lives in the most elevated regions of our globe. Its common length scarcely amounts to ten centimetres (four inches); but there are varieties which do not appear to reach five centimetres (two inches) in length.

In the system of ichthyology this new species of *pimelodus* should be ranged in the first sub-genus established by Lacepede, among the forked-tailed pimelodes. It must be in the first species, before the *pimelodus bagre*. As it is the only one of that division that has but two whiskers, I give it the name of

PIMELODUS Cyclopum. (Plate VIII.)

Cirris duobus, corpore olivaceo nigro-punctato.

This little fish lives in rivulets at the temperature of 10° of the centigrade thermometer, while other species of the same genus exist in rivers in the plains the water of which is at 27°. The *pimelodus* is but very rarely eaten, and then

only by the most indigent race of Indians; its aspect and the sliminess of its skin render it very disgusting.

From the enormous quantity of *pimelodes* that the volcanoes of the kingdom of Quito occasionally discharge, one cannot doubt that country contains great subterranean lakes which conceal these fishes; for the individuals that exist in the little rivers around are very few in number. A part of those rivers may communicate with the subterranean pits: it is also probable that the first *pimelodes* which have inhabited these pits have mounted there against the current. I have seen fish in the caverns of Derbyshire, in England; and near Gailenreuth, in Germany, where the fossil heads of bears and lions are found, there are living trouts in the grottoes, which at present are very distant from any rivulet, and greatly elevated above the level of the neighbouring waters. In the province of Quito, the subterraneous roarings that accompany the earthquakes; the masses of rocks that we think we hear crumbling down below the earth we walk on; the immense quantity of water that issues from the earth in the driest places during the volcanic explosions; and numerous other phænomena, indicate that all the soil of this elevated plain is undermined. But, if it is easy to conceive that vast subterranean basins may be filled with water which nourishes fishes, it is more difficult to explain how these animals are attracted by volcanoes that ascend to the height of 1300 toises, and discharged either by their craters or by their lateral vents. Should we suppose that the *pimelodes* exist in subterranean basins of the same height at which they are seen to issue? How conceive their origin in a position so extraordinary; in the flank of a cone so often heated, and perhaps partly produced by volcanic fire? Whatever may be the source from which they issue, the perfect state in which they are found induces us to believe that those volcanoes, the most elevated and the most active in the world, experience, from time to time, convulsive movements, during which the disengagement of caloric appears less considerable than we should suppose it. Earthquakes do not always accompany those phænomena. Perhaps, in the different concamerations that may be ad-

mitted in the interior of a volcano, the air is found occasionally condensed, and that it is this condensed air which contributes to raise the water and fish; perhaps they issue from a concavity distant from those which emit volcanic fire; possibly, in fine, the argillaceous mud in which those animals are enveloped defends them from the action of great heat. Notwithstanding all the researches that have been recently made on volcanoes, there is nothing but the study of volcanic productions that has made any progress. As to the nature of the combustibles which nourish those subterranean fires, and the mode of action of those fires themselves, I believe that all persons who have visited the borders of craters, and who have lived a long time in the vicinity of volcanoes, will sincerely avow, with me, that we are still very far from being able to give an explication, which, without being contrary to the principles of chemistry and of physics, could account for the great phænomena which volcanic explosions present.

The corregidor of the city of Ibarra, don José Pose Pardo, has communicated to me an interesting observation on the *pimelodes*. "It is known (says he, in a letter which I have still preserved,) that the volcano of Imbaburu, at the time of its great eruption on the side next our city, threw out an enormous quantity of *prennadillas*: it even continues still occasionally to do so, especially after great rains. It is observed that these fishes actually live in the interior of the mountain, and that the Indians of S. Pabla fish* for them in a rivulet at the very place whence they issue from the rock. This fishery does not succeed either in the day or in moonlight: a very dark night is therefore necessary, as the *prennadillas* will not otherwise come out of the volcano, the interior of which is hollow." It appears, then, that the light is injurious to those subterranean fishes, which are not

* This is an assertion somewhat contrary to that of their being very bad food, and disagreeable in appearance. It is within the particular knowledge of the translator, that the Spaniards of South America are both very sceptical and very witty, and that to play upon the philosophical faith of Europeans would be their highest delight. He must therefore be pardoned for regarding the letter of *el Senor Corregidor* as a *jeu d'esprit en revanche* for the sarcastic observations of French travellers on the Spaniards.—*Translator.*

accustomed to so strong a stimulus: an observation so much the more curious, that the *pimelodes* of the same species, which inhabit the brooks in the vicinity of the city of Quito, live exposed to the brightness of the meridian sun.

LXI. *Memoir on a new Species of Monkey found in the eastern Declivity of the Andes. By M.* DE HUMBOLDT*.

In the vast plains which extend from the eastern declivity of the Andes towards the shores of the Brazils, in the thick forests on the Amazons, Rio Negro (Black River), and Oronoco, the *cavia capybara*, *sus tajassu*, and the monkeys, are the quadrupeds most common. The marmose and alouate prevail over all the others, whether for variety of species or number of individuals. Some of these monkeys, such as the capuchin of Oronoco, very different from the *simia capucina* Linn., the tiger-monkey or cusicusi, and the widow, (three new species which I have discovered,) live in pairs, melancholy, mistrustful, flying (like man in a savage state) their proper species. Others, especially the sagouin and howling monkey, are seen in troops, from 80 to 100, springing from branch to branch in search of nourishment. The *saimiri* of Buffon, that are the *titi* of Atures (*simia sciurea*), so esteemed on account of their gaiety, their mildness, and extreme littleness, assemble together when it begins to rain. A fall of temperature of three or four degrees of the centigrade thermometer disturbs them so much, that they mutually embrace each other and form balls or knots, of which each individual seeks to occupy the middle, in order to find shelter. The Indian hunters, advertised by the cry of the titi, direct their arrows towards these flocks.

Notwithstanding the great number of monkeys that naturalists have described, it is probable that we are still ignorant of the tenth part existing. In Africa, and even in South America, there are vast plains of twenty thousand

* From *Recueil d'Observations de Zoologie et d'Anatomie comparé*, 1re livraison.

square leagues which have not yet been visited by any European. On the other hand, the monkeys the most common are still so imperfectly represented even in the most recent works, executed with the greatest elegance, that those who have seen the living individuals would have difficulty to recognise them in the drawings published. Of this I might cite as examples the *simia sciurea*, varieties of the *simia capucina*, and even the *simia paniscus*, which is the game commonly eaten in the Upper Oronoco.

Among the great number of new *sapajous*, or marmosets, that I had the opportunity of describing in my voyage to the tropics, I have chosen a monkey of the plains of Mocoa, remarkable for its resemblance with the lion of Africa, of which I made a drawing during my residence at Popayan. My sketch has been copied and improved by M. Turpin. (Plate VIII.)

The *leoncito** is very rare, even in its native country. It inhabits the plains which border on the eastern declivity of the Cordelliers, the fertile banks of the Putumayo and Caqueta: it never ascends even to the temperate regions, while the wandering bands of the *simia beelzebul* sometimes push their excursions to heights equal to those of Canigou, and even Mont Perdu. The *leoncito* of Mocoa, which I name *simia leonina*, differs essentially from all known species. It has not the white head of the *s. leucocephala* figured in the work of Audebert. It differs from the *s. rosalia*, and the saki or fox-tailed monkey (*s. pithecia*), by a white spot which covers the top of the nose, the mouth, and the chin; and from the *s. iacchus* of Brazil, by a tail without white rings, by its black visage, and by the disparity of conformation that exists between the claws of its fore-feet and the nails on its hinder ones; the former almost resembling the claws of a cat, and the latter having nails like the human toes. The *leoncito* is but seven or eight inches long, without counting the tail, which is of the length of the body. It is one of the least and most elegant monkeys that we have seen. It is gay and sportive, but, like most little

* *Leoncito*, from *leon* (lion) a diminutive of endearment more common even with the Spaniards than the Italians.—*Translator.*

animals,

animals, very irascible. When it is vexed it bristles up the hair on its neck, that increases its resemblance to the African lion. I have seen but two individuals of this very rare monkey: they were the first that had been brought living to the west of the Cordellier. They were kept in a cage; and their movements were so rapid and so continual, that I had much difficulty to design them. Their hissing imitates the song of little birds, and I suppose that the conformation of their larynx (having a particular sac) is analogous to that which I have described in the *simia œdipus*. I have been assured, that in the cottages of the Indians of Mocoa the leoncito breeds in the domestic state. By the way of Grand Para and the river of Amazons they might be brought into Europe. If a government, interested in the progress of descriptive natural history, would undertake an expedition in which that interest would not be rendered subservient to geographical discoveries; if that government sent canoes or small boats to ascend the Oronoco, and to penetrate by the Casiquiaré and Rio Negro to the river of Amazons; in short, if, after having explored the mouths of the Caqueta and Putumayo, it would make these same boats descend to Grand-Para, it would in a little time unite collections the most precious to the study of zoology and botany. Such an expedition would be of little expense, and its success certain.

SIMIA LEONINA.

Ex olivacco fuscescens, facie atra, ore albo, dorso striis albo-flavescentibus notato.

Caput parvum, depressum, nigrescens. Facies anthropomorpha, atra; macula albo-cœrulescens circa os et nares. Auriculæ subtriangulares distantes, margine superiori deflexo, magnæ, aterrimæ, pilosæ. Corpus ex badio olivaceum, pilis nigro-annulatis, in collo longioribus. Dorsum maculis et striis albo-flavescentibus variegatum. Cauda non prehensilis, longitudine corporis, superne atra, inferne badia, apice incurva et incrassata. Manus et pedes aterrimi, inferne nudi, pollice in manibus anterioribus et posterioribus distante. Ungues acuti, incurvi, atri; pollicis ungue in manibus anterioribus oblongo, acuto, in manibus posterioribus (pedibus) obtuso, anthropomorpho.

LXII. *Analysis of the Hot Springs at Bath. By Mr.* Richard Phillips, *Member of the Askesian and of the British Mineralogical Societies**.

The nature of the country round Bath, and other local circumstances, have been so fully described by those who have given chemical examinations of the waters of the hot springs at that place, that any further description appears unnecessary.

As to the cause of the heat of these springs, we have so few data from which to reason, that I shall not offer even a conjecture on the subject.

These waters have been frequently analysed. They have merited the attention they have received, not only from their early and extensive employment in the cure of diseases, but also on account of some peculiar changes to which they are subject. Of these the explanations have been so various as to show that they require still further examination.

Of the sensible properties exhibited by these waters the most remarkable is their high temperature, the degree of which varies considerably at their different sources. At the hot bath it is 117°; at the king's bath 114°; and at the cross bath 109°. This statement does not exactly agree with what has been usually given as their temperature. These results were obtained by pumping the water upon the bulb of a thermometer till the mercury ceased to rise. Their taste is metallic, but not strongly or disagreeably so; this has not been universally allowed: but if they are drunk hot, this impression is readily distinguishable.

Their specific gravity is 1·002 at each of the springs; and as the effects produced by chemical tests are also perfectly similar, they may be considered as derived from one source, the temperature varying by their more or less circuitous passage to the surface. For the purpose of analysis the water of the king's bath has been usually employed; and, although it does not appear to be a matter of much importance, I have followed the usual practice.

* From the unpublished Transactions of the Askesian Society.

Before

Before the experiments made upon the water are related, it will be necessary to state those employed to ascertain the properties of the gas, which rises in great quantity through the water in the king's bath.

This gas is perfectly free from smell.

(A) Some of the gas was received into a jar. A lighted taper put into it was immediately extinguished.

(B) Received into lime water, it caused an immediate precipitation.

(C) Tincture of litmus suffered no change of colour by agitation with the gas.

(D) The colour of dilute tincture of turmeric and infusion of galls was destroyed by it.

From these effects the gas appears to consist principally of nitrogen gas with a small portion of carbonic acid gas. To ascertain the quantity of each, and whether any oxygen gas was present, the following experiments were performed:

(E) One hundred measures of the gas were strongly agitated with barytes water in a graduated tube. A considerable precipitate was deposited, and five measures were absorbed.

(F) One measure of nitrous gas was added to an equal quantity of the gas in an eudiometer in the water apparatus. The mixed gases underwent no alteration of colour or diminution of volume.

(G) One hundred measures of the gas which had been deprived of carbonic acid by barytes water were submitted to the action of solution of green muriate of iron impregnated with nitrous gas. No absorption took place.

This gas, therefore, consists of,

Carbonic acid gas - -	5
Nitrogen gas - - -	95
	100

I now proceeded to try whether the water held either of these gases in solution.

(H) Ten ounces of the water, which had been cooled in a well closed bottle, were put into a vessel furnished with a

bent tube; the water was boiled for about twenty minutes, and the gas evolved from the water and the air of the tube, except a quantity too small to be estimated, were received in a graduated jar over mercury. Solution of potash absorbed 3-4ths of an inch of this gas, which was carbonic acid.

(I) The unabsorbed gas was transferred to the water apparatus, and tried in the usual way with nitrous gas. The mean result of comparative experiments upon it and atmospheric air showed that it was merely the air of the vessel, and that no nitrogen gas was held in solution by the water.

As ten ounces of the water gave ·75 of an inch of carbonic acid, one quart would furnish 2·4. This quantity is not quite exact, it being scarcely possible to obtain the whole of the carbonic acid by ebullition.

Effects of Atmospheric Air and Re-agents.

(K) Some of the water, while hot, having been exposed in a vessel of broad surface to the atmosphere, afforded in a few hours a small quantity of a white precipitate; but water which had been cooled in a closed vessel remained perfectly transparent after several weeks exposure to the air.

The re-agents added to the water while hot, and the effects produced by them, were the following:

(L) Acetate of lead,—a perfectly white precipitate.

(M) Tincture of litmus,—no alteration of colour.

(N) Tincture of turmeric,—no change indicating the presence of uncombined alkali; its colour immediately almost destroyed.

(O) Lime water,—a white precipitate.

(P) Ammonia,—a white precipitate.

(Q) Carbonate of ammonia,—a white precipitate.

(R) Some of the water was boiled with a little nitric acid, —ammonia added to this gave no precipitate.

(S) Oxalate of ammonia,— a precipitate.

(T) Nitrate of barytes,—a precipitate insoluble in nitric acid.

(U) Nitrate of silver,—a white precipitate insoluble in nitric acid.

(V) Sulphuretted

(V) Sulphuretted hydrogen water,—no precipitate or change of colour; the water became very slightly turbid.

(W) Prussiate of potash,—no immediate effect: after some weeks the water became slightly green.

(X) Infusion of galls,—immediately a peach-blossom red colour, and very soon a precipitate which became dark purple by exposure to the air.

All the above effects are also produced after the water has been cooled, excepting that the colour of tincture of turmeric is not then destroyed, and, under some circumstances, no red colour occurs upon the addition of infusion of galls.

(Y) A quantity of the water was evaporated to dryness, and distilled water added to the residuum. Nitrate of lime poured into the solution afforded a crystalline precipitate in a few hours, indicating the presence of an alkaline sulphate.

I shall now state the inferences to be deduced from these experiments.

Carbonic acid exists in this water (BE). A considerable portion of it escapes at the high temperature at which the water is obtained, its evolution occasioning the precipitation of some substance which it held in solution (K).

From Experiment (L) it is evident that no sulphuretted hydrogen gas is present.

As no alteration of colour is effected upon tincture of litmus by the carbonic acid (M), it is evident that acid is present only in sufficient quantity to dissolve the substance precipitated by its evolution.

The destruction of the colour of tincture of turmeric (N) is clearly occasioned by the gas during its passage through the water (D).

The effect produced in Experiment (O) is owing to the formation of carbonate of lime, and the precipitation of it and of the substance previously dissolved by carbonic acid (K).

A part of the precipitate obtained by adding ammonia (P) must have been similar to that of Experiment (K), and to a portion of that of Experiment (O), produced in (P) and (O) by combining the solvent carbonic acid instead of expelling

it

it as in Experiment (K). The precipitate was carbonate of lime, or of magnesia, or both.

As earthy carbonates are not precipitable by alkaline carbonates, the precipitate formed by carbonate of ammonia (Q) indicates the presence of some other earthy salt.

From Experiment (R) it appears that no alumina or magnesia exists in the water, and that the precipitate obtained in Experiment (K) was carbonate of lime. But according to Dr. Gibbes, sulphate of alumina is present. It appears, however, that Dr. Saunders is perfectly correct in supposing that it forms no part of the saline contents of the water. In addition to the experiments already stated, it may be shown to be incompatible with the salts actually existing in it; for the addition of a solution of sulphate of alumina occasions immediate precipitation.

As Dr. Gibbes has insisted on this point, I shall here mention what appear to have been the causes of his mistake. In reply to Dr. Saunders's supposition, that the precipitate which he took for alumina was carbonate of lime, he has stated that it was precipitated from the water by ammonia after oxalic acid had ceased to produce any further effect. This method is subject to error; for the oxalic acid appears to have been employed without previous combination with an alkali; and, as oxalate of lime is soluble in oxalic acid, it is evident that, if more of the latter were employed than was sufficient to precipitate the lime, it would dissolve a portion of oxalate of lime. The acid in combination with the lime previous to the addition of oxalic acid, not having an alkali to combine with, would dissolve a further portion of oxalate of lime; and unless the carbonic acid was expelled or saturated, it also would increase the error. From these circumstances I suspect that the precipitate afforded by ammonia was oxalate of lime, this compound being precipitable from acids by alkalies.

Experiment (S) determines the presence of lime.

Experiment (T) shows that sulphuric acid exists in the water.

The effect, produced by nitrate of silver (U) results from the action of muriatic acid.

As

As no metallic oxide, discoverable by sulphuretted hydrogen, was suspected, the appearance it produced (V) was supposed to be derived from its action upon carbonate of lime. This was ascertained to be the case by direct experiment.

The prussiate employed by Experiment (W) was the triple compound containing oxide of iron. It was imagined that the slight greenness which was assumed by the water might be occasioned by the action of the carbonic acid, notwithstanding its holding carbonate of lime in solution, this effect being easily produced by the application of the stronger acids. A small quantity of the triple prussiate was therefore added to a solution of carbonate of lime in carbonic acid: after a considerable time it acquired a green colour exactly similar to that observed in Experiment (W). Dr. Falconer has indeed stated that a blue colour is to be obtained by the action of prussiate of potash upon the water; but, as it did not occur till after the addition of sulphuric acid, it is evident that this effect was produced by the action of the acid upon the oxide of iron of the prussiate.

Although the presence of oxide of iron is not at all indicated by prussiate of potash, (probably on account of the smallness of its quantity,) yet it is evident from the action of infusion of galls (X) that a minute portion of it actually exists in the water; the light colour of the recent precipitate, and its becoming darker by contact with atmospheric air, showing that it is in the state of protoxide. In making this experiment it is requisite to use a very small quantity of the infusion of galls; for, if much more than five drops of it are added to one ounce of the water, no indication of oxide of iron is produced, the water becoming of a light reddish brown colour, and affording no precipitate. An excess of this infusion re-acts upon the compound of vegetable matter and oxide of iron so completely as to prevent the appearances readily presented by a small quantity.

From the well known laws of chemical affinity it is evident that the oxide of iron is combined with carbonic acid; this compound undergoing some curious changes, which have occasioned much discussion.

It

It has been observed, that one of the most active tests of oxide of iron does not in this water produce any appearance of its presence; and the slight metallic taste which it communicates when hot and fresh has been unnoticed by some analysts. This taste is lost by cooling, even in well stopped bottles; and every method which I have tried to restore it has been unsuccessful. It has also been mentioned that the action of infusion of galls is in some cases lost.

The proofs that this water contains oxide of iron thus existing under singular circumstances, and liable to cease, it is not surprising that they have been assisted by collateral evidence. It has been asserted that the water deposits "a pale yellow ochrey precipitate;" but this is certainly an error, the precipitate being perfectly white. Another circumstance which has been adduced to prove the precipitation of oxide of iron is, that the sides of the king's bath become encrusted with it: this observation, as to fact, is correct; but the oxide appears not to be deposited from the water, but derived from the stone, by the increased oxidation of the iron contained in it, by the alternate application of air and water. Having procured a specimen of oolite similar to that of which the sides of the bath are constructed, I added a drop or two of nitric acid to it; by this the iron became instantly and completely oxygenated, affording an appearance similar to that which has been supposed to be deposited from the water.

Another fact has been noticed equally deceptive with the above stated, which is, that the clothes of the bathers become stained with iron moulds. It is indeed true, that the bright yellow colour of the substance of which these clothes are made changes to a brown; but this change is not partial, nor has it any resemblance to iron moulds: it seems to be effected merely by the decay of the colouring matter, and I find that solutions of iron do not change the yellow colour.

The observation which has occasioned most discussion respecting the oxide of iron, is the loss of power of infusion of galls to detect it. The following experiments will show under what circumstances this occurs.

(*a*) About one pint of the water was exposed, while hot, to

to the atmosphere in a vessel of broad surface. After it had remained about 16 hours, a small quantity of carbonate of lime was deposited by the evolution of carbonic acid gas. The precipitate was perfectly white, and had not the slightest appearance of containing oxide of iron. To this water infusion of galls was added without occasioning the least alteration of colour.

(*b*) As the quantity of oxide of iron in the water is evidently extremely small, it may be imagined that it was precipitated with the carbonate of lime, but escaped observation from the minuteness of the quantity. To obviate this objection, some of the water was closely stopped in a phial for four or five days: upon examination it was found to possess its transparency perfectly, and without having afforded any precipitate; to some of this, infusion of galls was added without producing the slightest indication of oxide of iron.

(*c*) Some of the water which had been cooled so as to retain its transparency, was heated to its original temperature; infusion of galls was then added, but without producing any effect.

The facts exhibited in Experiments (*b*) and (*c*) have been long known, and have given rise to an idea that the iron is volatilized. Although this opinion is incompatible with facts already mentioned, yet it may not be amiss to show more particularly that it is completely erroneous. As it cannot be imagined that the temperature of the water is sufficient to volatilize mere oxide of iron, the existence of some substance capable of carrying it off must have been supposed. That muriatic acid and muriate of ammonia possess this power at high temperatures is well known, but no uncombined muriatic acid or muriate of ammonia is present. Hydrogen gas is said also to be capable of volatilizing iron; but the gas evolved from the water has been shown to consist of nitrogen gas and a small quantity of carbonic acid gas, and to neither of these gases, alone or combined, has any such power been attributed. If, however, they really possess it in this instance, they must be regarded as the solvent of the iron, and the effect produced upon infusion of galls must be derived

rived from the gas diffused in small quantity through the water. If this be the case, the application of the concentrated solution of iron should produce a much more distinct effect upon the infusion; but it has been shown (D) that the gas destroys the colour of infusion of galls instead of increasing it, which would be the effect if it contained oxide of iron.

(*d*) About one gallon of the water was put into a vessel of considerable depth, of which it occupied about two-thirds: it was slightly covered, and remained about twenty-four hours. It then retained its power of affording a peach-blossom coloured precipitate with infusion of galls (X) in a very considerable degree.

It is remarkable that in this experiment the result should have proved so different from that obtained in one where the circumstances were similar, excepting only the form of the vessel and the quantity of the water. When the water was exposed with a broad surface, infusion of galls showed no action on it (*a*); but here, even after eight hours longer exposure, it detected oxide of iron.

From this circumstance I began to suspect that some change was produced by the absorption of oxygen, and that it had not produced the same effect in this as in the former experiment, on account of the quantity of the water and depth of the vessel. There appeared, however, a strong fact against this supposition; viz. that iron is more easily detected when highly oxygenated, whereas the reverse effect in this case was produced.

To try the effect of atmospheric air, the following experiments were performed:

(*e*) A small quantity of the water was enclosed, while hot, in a well stopped phial, with about one-fortieth of its volume of atmospheric air. After four days the water remained perfectly transparent, but the addition of infusion of galls did not afford the slightest appearance of its containing iron.

(*f*) Another portion of the water was kept for the same length of time in a well stopped phial, but without any air except such as the water held in solution. Infusion of galls occasioned

occasioned exactly the same appearance of iron in this as in the water when fresh and hot (X).

That the action of infusion of galls is lost by the absorption of the oxygen of atmospheric air is proved by the following experiment:

(*g*) A third quantity of the water was enclosed, with the usual precaution, in a phial, about one-half of which was occupied by the gas evolved from the water in the bath, which has been shown to contain no oxygen gas. After four days, infusion of galls was added to it, and gave the same appearances of oxide of iron as occur in its application to the fresh hot water.

Having thus ascertained the effect of oxygen in preventing the action of infusion of galls upon oxide of iron, it remained to be shown in what manner this is effected. I imagined it might be produced by increasing the power of combination of the oxide of iron so as to admit of its acting upon the earthy contents of the water and forming compounds, the strong affinity of the constituents of which prevented the action of the infusion of galls. With a view to ascertain how far this supposition was correct, I examined the effects produced by adding carbonate of lime, dissolved by carbonic acid, to solution of sulphate of iron to which infusion of galls had been previously added; and although it will appear, by the following experiments, that the alterations produced upon the oxide of iron in the water are caused by the carbonate of lime it contains, it will also be found that they are not effected in the way I had supposed.

A very dilute solution of green sulphate of iron was prepared: the quantity of oxide of iron contained in it was so small as scarcely to afford any alteration of colour when infusion of galls was added to it; but upon pouring solution of carbonate of lime into it after infusion of galls had been added, a deep red colour was almost instantaneously produced.

Although this fact did not immediately appear likely to solve the difficulties attendant upon the water in question, yet it was sufficiently striking to merit an examination by what

what means the carbonate of lime produced this effect, and to what extent it might be employed in rendering infusion of galls a more active re-agent.

With this intention I boiled some crystallized sulphate of iron in alcohol till nearly the whole of the red sulphate was separated. The remaining quantity being extremely small, I shall consider the iron in this solution as entirely in the state of protoxide. The sulphate, insoluble in alcohol, was dissolved in water, and the quantity of the oxide contained in a given portion of the solution was ascertained by taking the average of two experiments.

(*h*) To one ounce of this solution, containing $\frac{1}{10\,0\,0}$dth of a grain of protoxide of iron, infusion of galls was added. This occasioned the usual appearances indicated by the presence of oxide of iron in a very slight degree. The colour produced, increased by the absorption of the oxygen of the atmosphere.

(*i*) An equal quantity of the solution was treated with prussiate of potash. A light blue colour was immediately produced by the minute portion of peroxide of iron which had escaped the action of the alcohol: the intensity of this colour was gradually increased by the action of atmospheric air till the iron had arrived at its maximum of oxidizement.

(*k*) Infuion of galls was added to one ounce of a dilute solution of carbonate of lime containing $\frac{1}{10\,0\,0}$dth of a grain of oxide, as in the former experiments. A red purple colour, of very considerable intensity, was immediately produced.

(*l*) The last experiment was repeated, employing only $\frac{1}{10\,0\,0\,0}$dth of a grain of oxide instead of $\frac{1}{10\,0\,0}$dth. A very distinct red purple was immediately produced by the action of the infusion of galls.

(*m*) To one ounce of a solution of carbonate of lime, containing $\frac{1}{10\,0\,0\,0}$dth of a grain of oxide, prussiate of potash was added; but it did not produce any indication of having acted upon the oxide of iron.

I now prepared a solution of red sulphate of iron by treating the green sulphate with nitric acid in a red heat. The quantity of oxide which the solution contained was as in the former

former case ascertained. The experiments made with this were as follow:

(*n*) One ounce of a solution of red sulphate of iron, containing $\frac{1}{1000}$dth of a grain of oxide, was treated with infusion of galls. The usual indications of its action upon oxide of iron were presented.

(*o*) The addition of prussiate of potash to an equal quantity of the solution immediately occasioned a blue colour.

(*p*) Infusion of galls was added to one ounce of a dilute solution of carbonate of lime containing $\frac{1}{1000}$dth of a grain of the peroxide of iron. Slight indications of its action upon the oxide were produced, but the colour was scarcely more intense than that effected by $\frac{1}{10000}$dth of a grain of protoxide in similar circumstances: no effect whatever was produced by infusion of galls upon $\frac{1}{10000}$dth of a grain of peroxide in one ounce of solution of carbonate of lime. The colour produced when carbonate of lime and infusion of galls are added to the peroxide is red purple, similar to that occasioned by their action upon the protoxide.

(*q*) To one ounce of a solution of carbonate of lime, containing, as in the last experiment, $\frac{1}{1000}$dth of a grain of peroxide of iron, prussiate of potash was added. Not the slightest blue colour was produced. When carbonate of lime was thus added to the solution of peroxide of iron, I found that it was capable of preventing the action of prussiate of potash upon $\frac{1}{250}$th of a grain.

From these experiments it is evident that carbonate of lime possesses, in a very great degree, the power of increasing the action of infusion of galls upon protoxide of iron; while, on the contrary, it diminishes its power in detecting peroxide of iron; and is, moreover, capable of preventing the action of prussiate of potash.

The application of these experiments to the circumstances of the water in question is obvious. It has been shown that it contains carbonate of lime; and that the power of infusion of galls to detect the oxide of iron it contains is completely lost by the absorption of oxygen. The following experi-

ment was made with the intention of trying whether this effect of slow oxidizement might be imitated:

(*r*) Infusion of galls is, as has been seen, capable of acting upon $\frac{1}{10000}$dth of a grain of protoxide of iron in one ounce of solution of carbonate of lime (*l*). A portion of sulphate of iron containing $\frac{1}{5000}$dth of a grain of protoxide was dissolved in one ounce of dilute solution of carbonate of lime, and was kept in contact, with about one-fourth of its volume of atmospheric air, during twenty-four hours. At the end of that time the solution remained perfectly transparent, and without having precipitated; but the addition of infusion of galls did not occasion the slightest appearance of having acted upon the oxide of iron. In this experiment the loss of power of infusion of galls is much more speedily effected than in the Bath water. This is evidently owing to the atmospheric air contained in the distilled water employed, while no oxygen gas is present in the Bath water.

When carbonate of lime is added to sulphate of iron it is well known that double decomposition takes place, the iron being thus combined with the carbonic acid instead of the sulphuric. Having found that infusion of galls, in several instances, acts much more readily upon carbonates than sulphates, I imagined that the carbonate of lime produced its effect in this way. To ascertain whether this supposition was correct I made the following experiment:

(*s*) A quantity of the solution of green sulphate of iron, similar to that employed in the above-related experiments, was decomposed by carbonate of potash; carbonic acid gas was passed through water in which the washed carbonate of iron was diffused, and to some of the filtered solution infusion of galls was added; but, instead of the red purple colour effected by the action of carbonate of lime upon sulphate of iron and infusion of galls (*k*) and (*l*), the usual deep blue colour was obtained.

(*t*) One-tenth of the quantity of carbonate of iron employed in the last experiment was dissolved in a solution of carbonate of lime equal in measure to the last solution. To this infusion of galls was added. The red purple colour

colour was immediately produced, and from its intensity it was evident that carbonate of lime had increased the power of infusion of galls as much in employing the carbonate as the sulphate of iron.

It may be concluded, from these experiments, that the effects produced by carbonate of lime are not attributable entirely, if at all, to the conversion of the sulphate of iron into a carbonate. Hence I was induced to examine the appearances produced by the action of the various alkalies and earths upon infusion of galls and solutions of iron. The results are by no means uninteresting, but the limits usually allotted to an analysis will hardly admit of the necessary detail; I shall therefore relate only such facts as appear requisite to explain the changes occurring in the Bath water, reserving the statement of the remaining experiments for a future opportunity.

I now proceeded to examine the salts produced by evaporating the water and crystallisation.

(*u*) A quantity of the water was evaporated to dryness: the residuum was treated with distilled water as long as that fluid continued to dissolve any portion of it. This solution was again evaporated, and upon cooling yielded a considerable quantity of acicular crystals. These were again dissolved in distilled water; and to a part of the solution nitrate of barytes was added, which occasioned a copious precipitate. The same effect was produced by oxalate of ammonia; but ammonia caused no precipitation. These crystals were therefore sulphate of lime. By further evaporation the solution afforded cubic crystals of muriate of soda and prismatic crystals of sulphate of soda.

The next object to be attained was the weight of the total quantity of the various substances held in solution by a given portion of the water. This has been given, with considerable variation, by different analysis, as will appear by the following statement. From a quart of the water

Dr. Lucas obtained 33¼ grains of dry residuum.
Dr. Charlton - 34
Dr. Falconer - 17¾
Dr. Gibbes - 23¼

To account for the great difference of these results, Dr. Saunders has supposed that the water varies at different times, or that the residuum has been dried with various degrees of heat. I have ascertained the quantity of the contents of the water several times during about eighteen months, without observing any other variation in its weight than is unavoidable in experiment. In support of this observation it may be remarked, that I found its specific gravity exactly as stated by Dr. Falconer.

It is scarcely probable that the results of any of these analyses were obtained by drying the residuum at a lower temperature than 212°, or at a greater than a red heat. Now I find that one quart of the water, weighing 30 troy ounces 172 grains, at the temperature of 63°, gives 32 grains of residuum dried at 212°: when the heat of a sand-bath is employed, 30 grains are obtained; and at a red heat, 28 grains. The greatest variation afforded by these methods is four grains; whereas from some cause, which it is difficult to explain, the extreme difference of the experiments above cited is 16½ grains. When a red heat is employed, a part of the loss is occasioned by the decomposition of the carbonate of lime; for water poured upon the residuum turns turmeric paper of a reddish brown colour. The greater part of the residuum is perfectly white; the portion deposited at the upper part of the vessel is, however, slightly greyish, but not at all appearing as if coloured by oxide of iron. I suspected that it might be occasioned by carbonaceous matter: to ascertain whether this was the case, the following experiment was made:

(*v*) Four pints of the water were evaporated to dryness in a retort, and the residuum boiled with about five ounces of alcohol. The filtered solution left, on evaporation, 8·3 grains of a yellowish-coloured substance. A part of this was dissolved in water, and afforded a copious white precipitate with nitrate of silver, but did not give any with ammonia or with carbonate of ammonia: muriate of soda was therefore the only salt dissolved by the alcohol.

(*w*) To the remaining portion of the saline mass colourless sulphuric acid was added. By heating, the acid acquired a dark

a dark brown colour, evidently derived from its action upon carbonaceous matter. This experiment did not appear conclusive, as two causes of error might have existed,—a small quantity of alcohol was probably decomposed by the action of the salts upon it, or some of the conferva which is found in the water might have escaped notice previous to evaporation. I had recourse, therefore, to other means. Mr. Kirwan, in his Treatise on the Analysis of Mineral Waters, gives a method for ascertaining the presence and quantity of extractive matter proposed by Westrumb, which consists in precipitating the muriatic salts by nitrate of lead, and afterwards the extractive matter by nitrate of silver. It is impossible to conceive any method more completely fallacious than this; for extractive matter is as readily precipitated by nitrate of lead as by nitrate of silver; and although muriate of soda is decomposed by nitrate of lead, muriate of lead being a salt of considerable solubility, the subsequent addition of nitrate of silver would decompose it, and afford a precipitate consisting of muriate of silver without any extractive matter.

The power of sulphuric acid to detect carbonaceous matter is extremely great: $\frac{1}{100}$dth of a grain of sugar was dissolved in four ounces of water; to this solution about one ounce of sulphuric acid was added: it was then boiled till nearly the whole of the water was evaporated, and the acid had acquired a very distinct brown colour.

The following experiment was now made:

(x) A quantity of sulphuric acid was added to one quart of the water perfectly transparent, and free from heterogeneous matter. The mixture was evaporated nearly to dryness in a retort, and the acid remained perfectly colourless. The water, therefore, contains no carbonaceous matter.

The substances contained in the water, as shown by the foregoing experiments, are; carbonate of lime, oxide of iron, sulphate of lime, muriate of soda, and sulphate of soda. The presence of these compounds has been universally allowed; but that silica is contained in the water, was discovered by

Dr. Gibbes. To find the quantity of each of these, the following methods were employed:

(*y*) A quart of the water was evaporated to dryness in a platina crucible: the residuum, dried in a sand heat, weighed thirty grains. This was boiled, with successive portions of distilled water, till it ceased to afford a precipitate with nitrate of barytes. The solution was then divided into three equal quantities.

(*z*) To one of these portions nitrate of silver was added as long as precipitation took place, and distilled water was poured on the precipitate till it came away quite pure. The muriate of silver thus obtained was weighed after exsiccation.

(*A*) The second quantity was treated with oxalate of ammonia while it continued to produce any effect. The precipitated oxalate of lime was washed, dried, and weighed.

(*B*) To the remaining part of the solution nitrate of barytes was added till it ceased to produce any precipitate; and the sulphate of barytes obtained by its action was weighed, after washing and drying, as in the former experiments.

(*C*) The residuum, insoluble in water, weighed, when dried, two grains: nitric acid added to it dissolved 1·7 grains. This solution afforded no precipitate with ammonia, but a copious one with oxalate of ammonia: it was therefore nitrate of lime obtained by the decomposition of the carbonate.

(*D*) The ·3 of a grain left by the nitric acid was dissolved by potash, and precipitated from it by muriate of ammonia. This precipitate was not again soluble in nitric acid, and was consequently silica.

Another quart of the water was treated in the same way. To avoid prolixity, I shall state the quantity of each precipitate afforded by one-third of a quart multiplied by three, and make the requisite calculations from the mean of the two experiments.

	Exp. I.	Exp. II.	Mean.
Residuum	30· grains	30·	30·
Muriate of silver	16·2 do.	16·2	16·2
Oxalate of lime	18·3 do.	17·7	18·

Sulphate

	Exp. I.	Exp. II.	Mean.
Sulphate of barytes	36·6 grains	36·9	36·7
Carbonate of lime	1·7 do.	1·5	1·6
Silica	·3 do.	·4	·35

According to Dr. Gibbes, a quart of the water affords nearly 4 grains of silica when treated in the method I have described. Thinking it probable that a portion of it might be taken up by the action of the salts during their solution in water, I tried whether any larger quantity could be obtained by the following method:

(*E*) A quart of the water was evaporated to dryness in a platina crucible. The residuum was repeatedly treated with nitric acid in a red heat; the soluble parts were again dissolved by distilled water, and the portion insoluble in it, when dried, weighed ·4 of a grain. This agreeing exactly with the last experiment, I shall consider as the quantity of silica afforded by a quart of the water. This experiment was several times repeated, with very little variation in the weight of the result, but was sometimes evidently coloured by oxide of iron, which was separated from the silica, and its nature ascertained by the usual means. But, even when employing apparently perfectly similar means, the oxide of iron was not always to be obtained,—an effect attributable to the decomposition of the muriate of soda by the nitric acid, and to the power which muriatic acid possesses of carrying off oxide of iron; but for the uncertainty of its action it is not easy to account.

To find the quantity of oxide of iron contained in the water, the following means were employed:

(*F*) To a quantity of the hot water infusion of galls was added in the requisite proportion. The water measured when cold 9½ pints. The precipitate obtained was separated by the filter, and dried:—the precipitate and filter were then burned together in a platina crucible, and the carbonaceous matter of the filter, and that combined with the iron, were got rid of by the application of a red heat. The residuum was then treated with nitric acid, in order completely to oxidize the iron:—it was then boiled with acetic acid to

take up the lime precipitated with the oxide of iron by the infusion of galls; and afterwards with potash, to dissolve any silica which the filter might have furnished—the remaining substance was evidently oxide of iron, and weighed ·2 of a grain.

(*G*) The last experiment was repeated, slightly varying the method. Infusion of galls was added, as before, to a quantity of the hot water, measuring after it had cooled 17½ pints. The precipitate was suffered to subside, and the water poured off till only a small quantity remained. This was evaporated, and the residuum, treated with nitric acid in a red heat, weighed ·5 of a grain. Being exposed to a red heat with carbonaceous matter, it became magnetic, and dissolved in muriatic acid, except $\frac{1}{10}$th of a grain, which appeared to be silica, derived from the water evaporated to obtain the precipitate formed by infusion of galls. The muriatic solution afforded a blue precipitate with prussiate of potash, ·4 were therefore oxide of iron.

According to the experiment (*F*) one quart of the water affords ·00421 of a grain of oxide of iron, and by the second ·00463, giving a mean of ·00442; but the iron in the water is in the state of protoxide; and as the peroxide consists of 66·5 protoxide, and 33·5 oxygen, ·00442 will give ·00394, the quantity of protoxide of iron in one quart.

242 of muriate of silver indicate 100 of muriate of soda, 16·2 will therefore give 6·6.

100 of oxalate of lime are produced by 100 of sulphate of lime, 18 will then give 18.

100 of sulphate of lime afford 175 of sulphate of barytes, 18 will then produce 31·5, which subtracted from 36·7, the whole quantity of sulphate of barytes obtained, leave 5·2 for the sulphate of barytes formed by the sulphate of soda; and as 100 of sulphate of soda give 170 of sulphate of barytes, 5·2 yield 3.

One quart of the water therefore contains

Carbonic acid	-	2·4 inches.
Sulphate of lime	-	18· grains.
		20·4

Brought

Brought forward	-	20·4
Muriate of soda	-	6·6
Sulphate of soda	-	3·0
Carbonate of lime		1·6
Silica -	-	·4
Oxide of iron	-	·00394
		29·60394
Loss	-	·39606
		30·

Estimating the muriate and sulphate of soda in the crystallised state, one pint of the water contains nearly as follows:

Carbonic acid	-	$1\frac{1}{5}$ inch.
Sulphate of lime	-	9 grains.
Muriate of soda	-	$3\frac{1}{2}$
Sulphate of soda	-	$3\frac{1}{2}$
Carbonate of lime	-	$\frac{8}{10}$
Silica -	-	$\frac{1}{5}$
Oxide of iron	-	$\frac{1}{50}$

LXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

MAY 1. The right honourable the President in the chair.—On resuming the reading of Mr. Smithson's letter (not Tennant, as erroneously printed in our last report) it did not appear that the author had been able to collect any quantity of the *native* minium which he discovered in galena; nor does he mention in what country he found this mineral.

An anatomical dissertation, by Mr. Home, on the *teredo gigantea*, or sea-worm, with observations on the *teredo navalis*, or borer, was also read. The subject examined by this anatomist was one of those sea-worm shells brought by Mr. Griffiths from Sumatra, of which some account was given to the Royal Society in February last. The *teredo navalis* was brought from one of our ships at the Nore, in order

der to enable the author to pursue his inquiry into the comparative anatomy of these animals. In the stomach of the borer he found a considerable quantity of matter, which Mr. Tennant pronounced to be purely ligneous, and similar to saw-dust. Notwithstanding this circumstance, and that of the borer's burying itself always in timber, Mr. Home concludes that wood is not properly its food, as it passes undigested; but that the animal, as well as the *teredo gigantea*, draws its nourishment from the water.

May 8. The President in the chair.—On this evening was read a letter from Mr. O. Gregory, of the Royal Military Academy at Woolwich, on sir Isaac Newton's definition of the force of the lever, in which the author defended sir Isaac's demonstration as the most simple and correct, against all the objections urged by other mathematicians. Mr. G. reduced his defence to propositions illustrated by figures, and maintained that the force of a lever of the same length and weight was equal in whatever other direction it might pass from the fulcrum, as well as horizontal.

May 15. The President in the chair.—An interesting letter to the President, from Mr. T. A. Knight, on the inverted action of the alburnous vessels of plants, was read. Mr. Knight, discovering some facts in opposition to the sentiments of Hales and Duhamel, has taken much pains to confirm them by experiments, all of which have tended to prove the truth of the opinions and observations which he has communicated from time to time to the society, relative to the circulation of the juices in plants, formation of buds and sap-wood, and to vegetable physiology in general. In the present case his researches were directed to the roots of potatoes, and he in consequence maintains that the formation of all such roots is explicable only on the principle which he has been endeavouring to establish, that of the *inverted* action of the alburnous vessels in plants. His experiments seem to have been conducted with his usual accuracy, and leave little doubt of the truth of his observations and opinions. Perhaps the discoveries of this ingenious philosophical botanist may be found directly applicable to the use of hot-house or green-house plants, in directing the gar-

dener's

dener's attention to the supply of that kind of air most easily assimilated by the lungs or respiratory organs of the different species of bulbous- and tuberous-rooted plants, and thereby find a new *pabulum vitæ vegetabilis.* It is, however, certain that we are yet very ignorant of the indefinite improvability of vegetables, and of the best means of multiplying them for the use of animal life.

May 22. The President in the chair.—A letter from Mr. Martin to the right honourable Mr. Greville, gave a brief description of a kind of mineral bason, or surface of a country, extending 100 miles in length, and 20 in breadth, through Wales, in Glamorgan and adjoining shires, which consists principally of iron ore and coals. Brief as this mineralogical topography is, it were to be wished that we had similar sketches of all the districts in Great Britain, as we might thence form more accurate ideas of the geology of the kingdom.

A letter from Mr. Dunlop to sir C. Blagden, bart. was likewise read, containing a description of an instrument newly invented by him, to be substituted for the common quadrant and sextant now in use, and by means of which the numerous errors occasioned by atmospheric refraction, and in taking lunar observations, &c. are to be completely obviated. Should this invention be found to answer these pretensions, it will be of inestimable consequence to navigators, as such is the imperfection of our present instruments, that no two observers can bring the sun to the same altitude on the quadrant, often differing from 4 to 7 miles in their observations: an endless source of errors and miscalculations in the course of a long voyage.

SOCIETY OF ANTIQUARIES.

May 1. The lord bishop of Salisbury in the chair.—A half-length portrait of sir Walter Raleigh was presented to the society by sir W. Skeffington, bart. In the letter which accompanied this present, it was acknowledged that the painting was only a tolerably old copy of some original: the upper part indeed was very well executed, but the hands were extremely incorrect and unnatural.

Designs

Designs of two towers in the East Indies were also exhibited: the one was alleged to be 2000 years old, and the other 700. They afford nothing peculiar, or indicative of having any allegorical import.

May 8. Craven Orde, Esq. vice-president, in the chair.—Eight plans, elevations, and sections of the cathedral church of Norwich, by Mr. Wilkins, of Cambridge, were exhibited. The society purchased these drawings for 150*l*. Several autograph letters from Henry VIII., sir Walter and lady Raleigh, were read. The letters signed E. Raleigh are written in a very fair character, and very small, except that which requested attendance at the funeral of her husband, sir Walter, in which her perturbation of mind is apparent in the unusual largeness of her hand-writing. These letters also evince great powers of mind; and the expression, speaking of the execution of sir Walter, that "the lords have not refused me his body, though they refused me his life," has here a more than ordinary energy and sarcastic reflection.

May 15. Craven Orde, Esq. in the chair.—Silver and copper Lydian coins, bearing a head of Jupiter, were exhibited, and considered as very rare. An antient parish seal, which contained a private seal within it, was also laid before the society. Some more drawings of Mr. Weston's Greek coins were exhibited. This gentleman has changed his ideas relative to a system of antient geography, which he seemed disposed to create in order to support the imaginary importance and antiquity of his coins; he will perhaps find it necessary to reconsider his opinions a second time, and may in consequence again change.

May 22. The Rev. Dr. Hamilton in the chair.—After reading the minutes of last meeting, the society adjourned till the 5th of June.

SOCIETY FOR THE IMPROVEMENT OF ARTS, MANUFACTURES, AND COMMERCE.

Tuesday the 27th of May having been the anniversary of this truly useful and patriotic society for the distribution of premiums

premiums and rewards, at their house in the Adelphi, his grace the duke of Norfolk, president, took the chair; and the meeting was honoured by the presence of the baroness de Tott, count Woronzow (late ambassador from Russia), the Swedish, Portuguese, and Hessian ambassadors; and as many persons of distinction, and ladies of fashion and beauty, were also present as the great room of the society could hold, to the exclusion of many of the members, who handsomely gave up their places on this occasion. At twelve o'clock the chair was taken, and the business commenced with an oration by Dr. Charles Taylor, the principal secretary, on the patriotic views and labours of this society since its establishment in 1754; in the course of which he noticed the principal objects of public utility, which the premiums and bounties, from time to time given by the society, had brought to light and perfected; and entered into minute details on the design and objects of the premiums awarded in the present session. The worthy secretary observed, that the permission which the society gave in 1760 for the artists of that day to exhibit their pictures in the society's rooms, gave rise to that noble annual exhibition, since made in the Royal Academy's apartments in Somerset-house; and he took the opportunity of paying a just tribute of respect to the memory of the late Mr. James Barry, the sublime productions of whose pencil decorated the society's room, and whose remains were now deposited in St. Paul's cathedral, between those of sir Christopher Wren and sir Joshua Reynolds.

After the different premiums, &c. had been adjudged, his grace rose, and, in a short speech, thanked the company for their polite attention to the details of the business; and, adverting to the distinguished foreigners present, concluded with a handsome compliment to the attention paid by the Emperor of Russia to the arts of peace, and particularly to the talents and productions of Englishmen.

In our next number will be found a list of the names of those to whom medals or premiums were delivered at the above anniversary.

IMPERIAL

IMPERIAL RUSSIAN SOCIETY OF THE ARTS AND SCIENCES AT ST. PETERSBURGH.

This society is at present employed in making arrangements for the future use of the Roman letters in writing and printing the Russian language. Accounts from St. Petersburgh speak highly of the rapid progress of literature through all the Russias.

ROYAL SOCIETY, COPENHAGEN.

M. Mœller, director of the grammar school at Slagelse, in Denmark, has this year received the gold medal from this society for the best answer to the following historical prize question: "Upon the misapplications which both antient and modern historians have made of what are called pragmatical histories, and on the means of preventing such mistakes."

THE ROYAL ACADEMY, STOCKHOLM.

This academy has announced the following prize subjects for the present year:

History—"Historical remarks upon taxes and imposts, and the manner of raising them in Sweden, during the middle ages." The prize is a gold medal of 26 ducats.

Greek, Latin, or French Languages—"On the resemblance of the Latin writers of the two epochs generally known by the appellation of the golden and silver ages; and examination whether the varieties in style and taste which characterize these two periods in the Roman literature, may not be attributed, as well as the varieties in modern literature, to the progress of society, and improvement of the human mind." The prize is a gold medal of 26 ducats.

The essays must be transmitted to Stockholm on or before the 20th of January 1807.

ATHENÆUM AT POICTIERS.

On the 31st of August, 1805, the Athenæum at Poictiers held a public meeting. The president, professor Fradin, opened it with a discourse on the utility of the belles lettres and philosophy. Various treatises were then read by the different

different members. One of these was an essay by M. Moreau, physician at Rochefort, on the property of charcoal in purifying water. Another essay was read by professor Fradin, on life in general, and particularly on old age: observations were also read by Mr. Boncenne, upon the fineness of taste and feeling: and M. Denesle, lecturer on botany, concluded with a treatise on the waters of Poictiers and the neighbourhood.

UNIVERSITY OF GOTTINGEN.

The following is announced as the subject of a prize question by this university: "What influence have oxygen gas, azotic gas, and the other gases, or their bases, upon the production of electricity by friction?" The beginning of September next is fixed for receiving the answers to the above, and the prize is 50 ducats.

MEDICAL SOCIETY OF BRUSSELS.

This society, since its re-establishment in the year 1801, has been employed in medical and philosophical researches of considerable importance. Two gold medals are given annually, the one of the value of 200 franks, for the best treatise upon any general medical subject; and the other, of 150 franks in value, for the best treatise upon any local disease peculiar to the department of Brussels. The subject of the prize essay for the year 1805, wa, "Whether has night any influence upon sick people; and what are the physical causes of such influence?" The prize was adjudged to Richard de la Prade, M.D. practising at Monbrison. The prize announced for the present year is for the best answer to the following question on a general medical subject: "What are the characteristic symptoms of the inflammation of the mucous system, and what phænomena are the consequences of such inflammation in regard to the organs in which it takes place; and what treatment ought we to follow in such a case?"

The prize will be adjudged in the public sitting of the 22d of September 1806.

LXIV. *In-*

LXIV. *Intelligence and Miscellaneous Articles.*

RUSSIAN EMBASSY TO CHINA.

Riœchta, Feb. 12, 1806.

After the extraordinary demonstrations of friendship made by the Chinese upon the arrival of our embassy on the frontiers of China, we heard with astonishment that the Chinese commander at Wiga, after admitting our ambassador, count Golowkin, into that place, had prevented any part of his suite from following him. The return of count Golowkin is therefore daily expected, unless the court of Pekin shall give a favourable answer to the remonstrances of our court.

Petersburgh, April 25.

The differences between our ambassador, count Golowkin, and the Chinese, who have sent a deputation to adjust certain ceremonies, are expected to be soon settled.

AEROSTATION.

The balloon of the unfortunate aëronaut, M. Mosment, whose melancholy fate was mentioned in our last number, came to the ground on the same day of the accident, 25 leagues distant from Lisle, the place of its ascension. An unloaded pistol, a little bread, and a piece of flesh, were found in the car of the balloon.

The unfortunate death of M. Mosment has produced the following observations from Garnerin, the aëronaut:—"It seems that the misfortune which happened to M. Mosment did not result from any of the inconveniencies which are connected with aërostatic ascensions, but merely from want of prudence. The car in which he ascended was too shallow; the cords by which it was attached to the balloon were too far apart; and it is probable, when M. Mosment was leaning over to let an animal drop in a parachute, that he lost his balance, and was precipitated to the earth. If the accidents which have happened from balloons are investigated, it will be seen that they have in general proceeded from the imprudence of the aëronauts themselves. Every one foresaw, when the Montgolfiere was attached to a balloon filled with inflammable gas, the danger to which

Pilatre

Pilatre de Rosier was exposed. Besides, those balloons were gilt; which might attract the electricity of the clouds. Balloons, gilt or silvered over, are very dangerous. Zambeccari, who employed those means, sustained several accidents, and it is only surprising that he escaped at last."

COFFEE.

M. Tussac, a colonial refugee from St. Domingo, has discovered the method of extracting from the pulp of coffee-berries a spirituous liquor similar to rum, and remarkable for a peculiar flavour which indicates its origin. This discovery will be very useful in the colonies, as the pulp, when separated from the berries, was hitherto used only as dung; and also because it may be substituted, to good account, in place of the rum and the tassia in common use. M. Tussac has sent a bottle of this liquor to the Museum of Natural History at Paris, and all who tasted it have declared that it was excellent.

He sent at the same time the model of a machine called a kiln, with the assistance of which the same quantity of coffee grains may be dried in three days, as could have been dried in six weeks if spread upon hurdles. This machine is a species of barrel or cylinder, the sides of which are constructed of brass wires, fastened to iron-rods; it admits of being easily seen through; it is divided into six or eight compartments, and a wooden axle runs through its whole length, to the extremity of which is adapted a handle. It is placed in a stove some feet above the surface, then half filled with coffee grains, and turned round by means of a horse or water mill. As this large box turns round, the coffee grains contained in it keep continually moving about; the partitions or compartments in it prevent the coffee from going too much to one side; the warm air of the stove passes through the intervals, the humidity is dissipated, and the dried clay or sand is easily separated.

M. Tussac has it in contemplation to publish a history of the vegetables of the Antilles, in which he purposes to give the process of distilling spirits from coffee, as well as a description and a drawing of the above kiln for drying the grain.

MISCELLANEOUS.

The Swedish astronomer Svanberg has this year received the prize bequeathed by Lalande, to be given annually for the best work on astronomy.

M. Leonhard, of Hanau, near Frankfort on the Main, has announced an intention of publishing annually a work on mineralogy, upon an extensive scale. He solicits original communications, and purposes publishing his first volume on the 1st of January, 1807, under the title of "A Mineralogical Pocket Book."

Several of the medical literati on the continent are at present engaged in making inquiries and experiments upon the influence of music in the cure of diseases.

A lying-in hospital has been established at Halle, in Saxony, upon an extensive and liberal plan. The subscription for its erection has been filled, and a large fund is also provided for its annual support.

The editors of the *Gazette de Santé* have announced a premium of a gold medal, of the value of 200 franks, for the best answer to the following questions: "What are the proximate causes of epidemics? Do they depend upon various miasmata which are conveyed through the medium of the air, or are they occasioned by actual contact? Or do they depend merely upon the qualities of smells? Is it clear that all stimulants are preservatives against infection?"

The answer may be written in Latin, French, English, Italian, Spanish, or German, and must be given in on or before the 17th January, 1807.

The emperor Napoleon has purchased, from his brother-in-law prince Borghese, the famous Borghese Villa and all its contents at the price of 13 millions of franks: the valuable collection of antiquities it contained are to be transported to Paris in order to enrich the Napoleon museum.

TRAVELS.

TRAVELS.

A letter from Goree, dated 3d March, 1806, gives the following information respecting Mr. Park: "We have just received information from the interior, that Mungo Park has been some time on the banks of the Niger, but could not build the boats as he intended, his carpenters being all dead, together with all the soldiers of our corps (35 in number) who went with him, except seven; who, with Mr. Park, Lieutenant Martyn of our corps, and a Mr. Scott an artist, have proceeded in canoes down the Niger; a brother-in-law of Mr. Park (named Anderson) went with him, as surgeon, but died on the banks. They all left Goree in April last. I saw the negro who brought the information, and have every reason to believe he is correct. Should you meet with any of Mr. Park's friends, this news may be acceptable to them. About seven weeks since he was seen in good health."

The counsellor of the mines of Denmark, M. Giesecke, who lately returned to Copenhagen from his voyage to the Faro Islands, has received permission from his Danish majesty to set sail for Greenland, where he will undertake a mineralogical and geographical inquiry into the state of that extensive country.

LONGEVITY.

In the list of persons who died in the year 1805, in the heptarchy of Pinsask, in Russia, five of them were 110 years of age; one of 113; four of 120; one of 128; one of 130; and one of the uncommon age of 150 years!

CHEMISTRY.

Mr. Parkes's Catechism, for the use of those who are entering upon the study of chemistry, and which we announced in our Number for December last, is now published in one volume octavo.

LECTURES.

On Monday, June 2d, a Course of Lectures on Physic and Chemistry will recommence at the Laboratory, Whitcomb-street, Leicester-square, at the usual morning hours:

viz. Therapeutics at a quarter before, the Practice at half after 8; and the Chemistry at a quarter after 9; by George Pearson, M.D. F.R.S. senior physician to St. George's Hospital, &c. &c.

The Practice of Vaccination will be taught at the Institution, 44, Broad-street, Golden-square, according to the plan proposed in a former Number of our Journal (May 1803).

Further particulars may be had at St. George's Hospital, or at 52, Leicester-square.

LIST OF PATENTS FOR NEW INVENTIONS.

To Anthony Francis Berte, of the parish of St. Dunstan in the West, in the city of London, merchant; for a machine for casting or founding types, letters, and ornaments, usually made use of in printing. April 29, 1806.

To William Bundy, of Pratt Place, Camden Town, in the parish of St. Pancras, in the county of Middlesex, mathematical-instrument-maker; for a machine or instrument for the purpose of making leaden bullets and other shot. May 1, 1806.

To Stephen Hooper, of Walworth, in the county of Surrey, gentleman; for an aqueduct tunnel, or machine for cleansing docks and other basons of penned water, and certain improvements on machines or machinery (for which he hath already obtained letters patent) for cleansing dry and other harbours, rivers, creeks, bars of harbours, and other purposes. May 3, 1806.

To William Robert Wale King, of Kirby-street, in the parish of St. Andrew, Holborn, in the county of Middlesex, tin-plate worker; for his new improved method of manufacturing tin or iron plates covered with tin, commonly called tin plates, into covers for dishes and plates. May 8, 1806.

To Martin Cawood, of Leeds, in the county of York; for an improvement in the manufacturing metallic cocks, for conveying and stopping liquids. May 15, 1806.

To Richard Willcox, of the parish of St. Mary, Lambeth, in the county of Surrey, mechanist; for certain improvements in steam-engines. May 21, 1806.

METEORO-

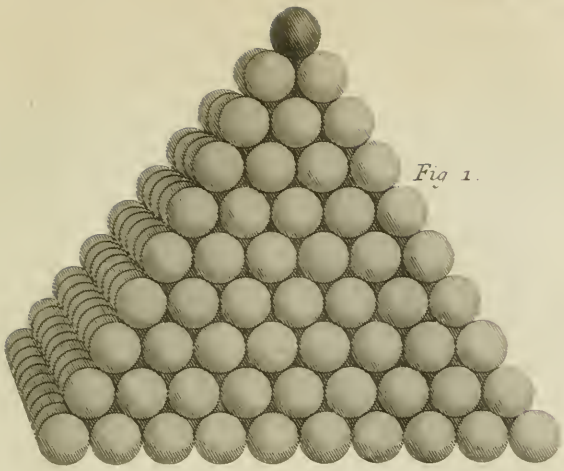


Fig 1.

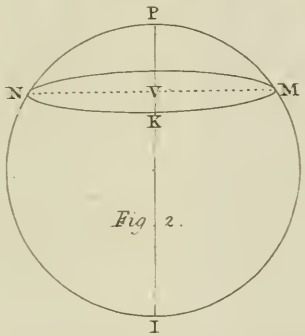


Fig 2.

Horizontal view of Air in Water.
Incumbent Stratum of Atmosphere Particles marked thus. •
Stratum of Air in Water marked thus ×

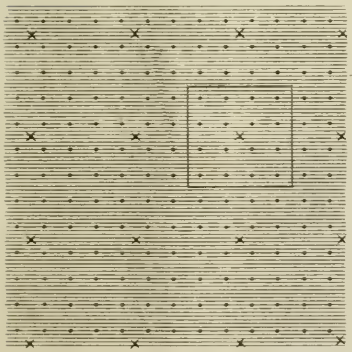
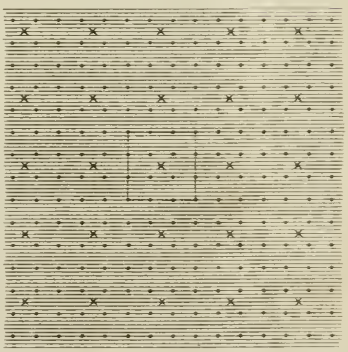


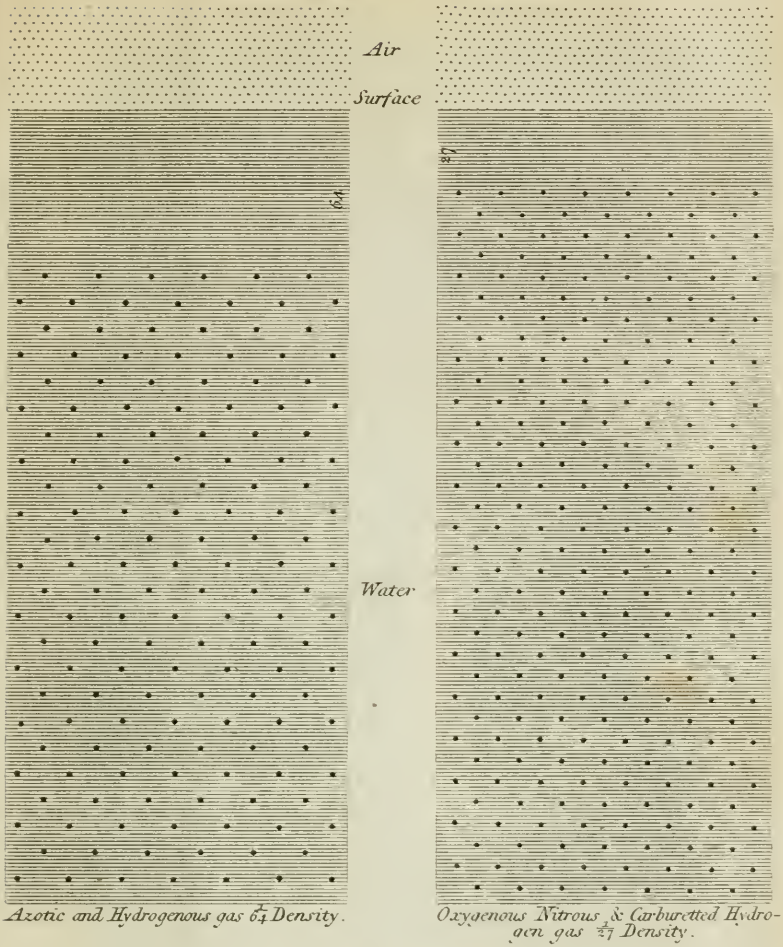
Fig. 3.



Azotic & Hydrogenous Gas
Distance of Particles 4 to 1.

Oxygenous Nitrous & Carburetted Hydrogen
Gas.—— Distance of Particles 3 to 1.

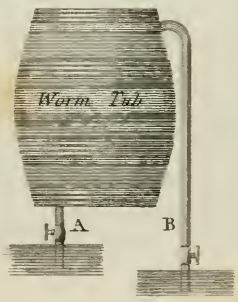
Profile View of Air in Water.



Azotic and Hydrogenous gas $\frac{1}{64}$ Density.

Oxygenous Nitrous & Carburetted Hydrogen gas $\frac{1}{27}$ Density.

Fig. 2.



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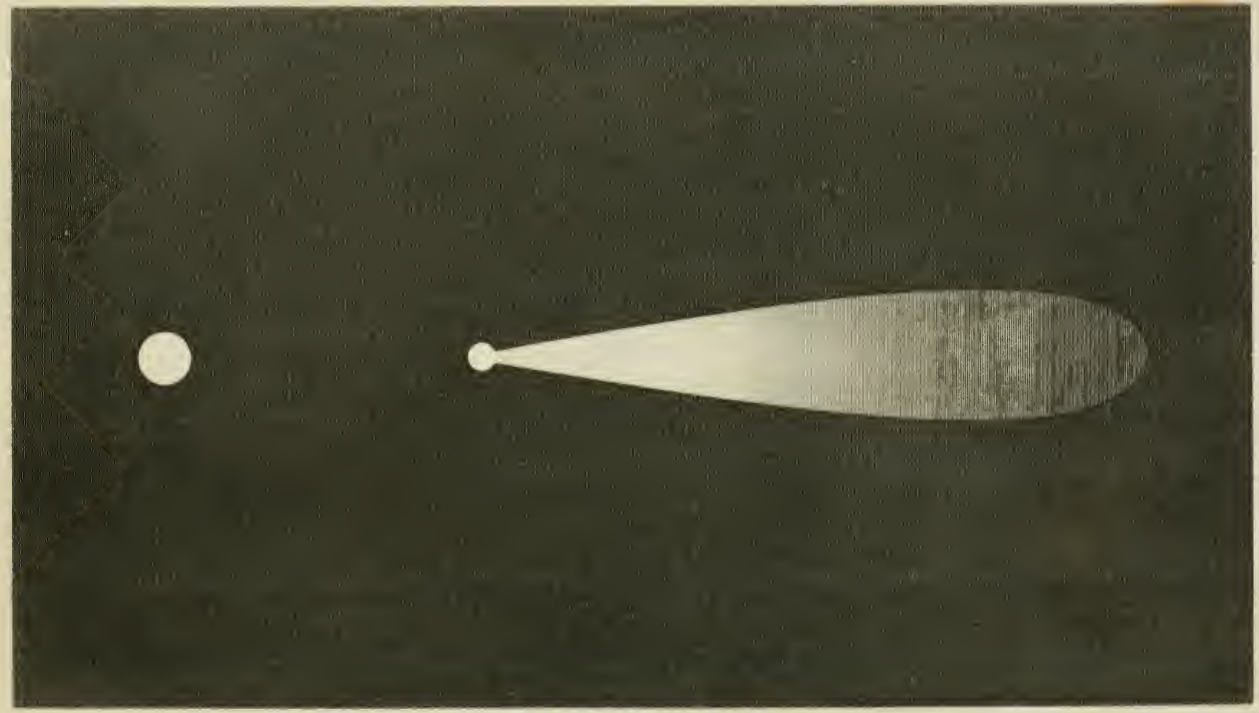
Mr Walker's Planetarium.



Lowry sculp

Fig. 8.

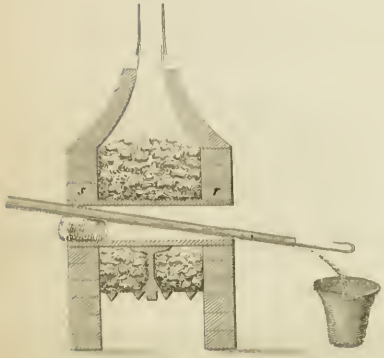
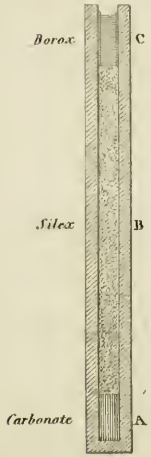


Fig. 9.



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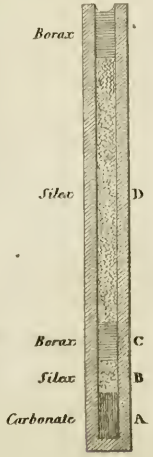

Fig. 11.

Fig. 12.

Fig. 13.

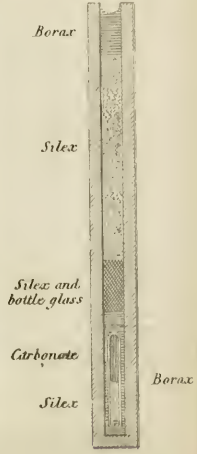

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Fig 1

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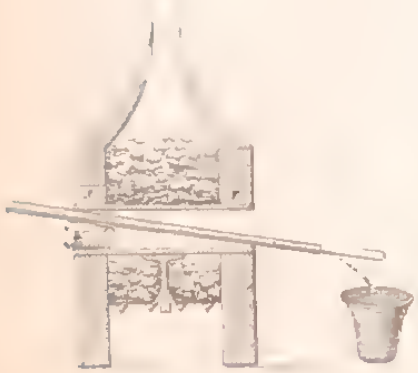

Fig 9

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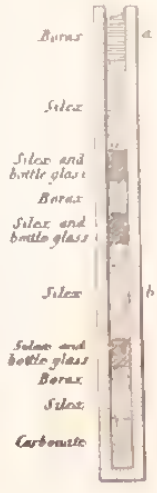


Fig 13

Mr. Steevens's Gazometer.

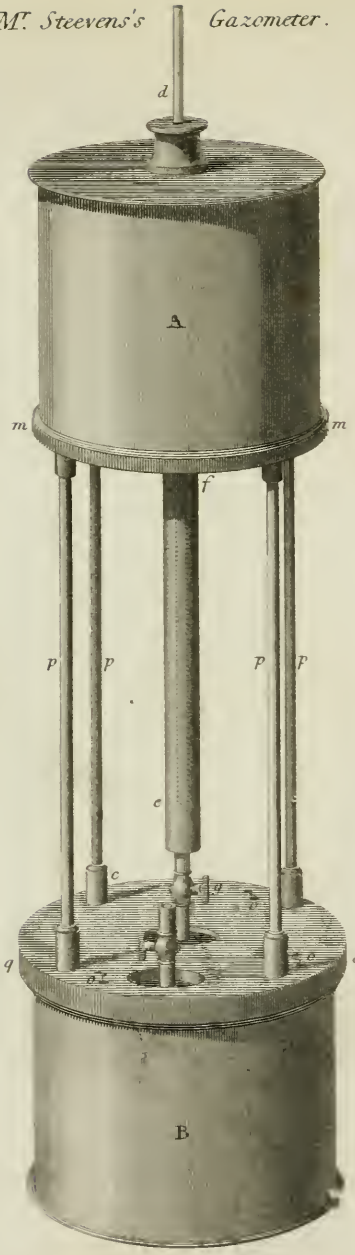
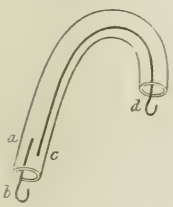


Fig. C.



Lowry sculp

Illustration of the Phænomenon of the Horizontal Moon.

Philo. Mag. Pl. VI. Vol. XXIV.

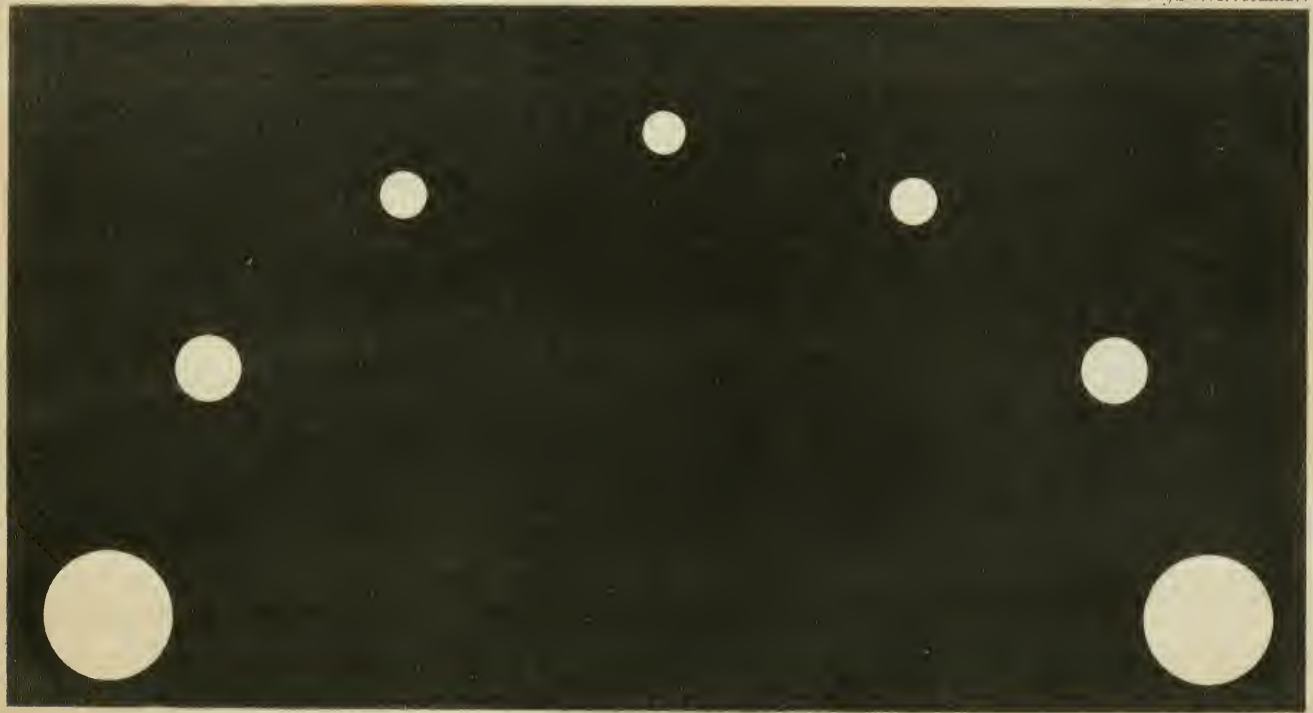


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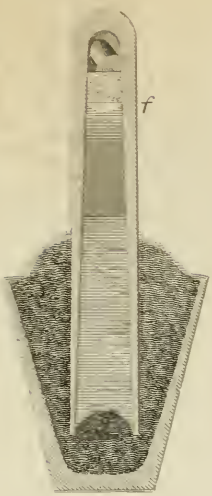
Fig. 17.



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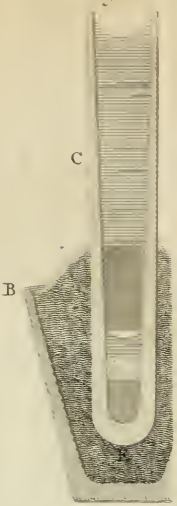

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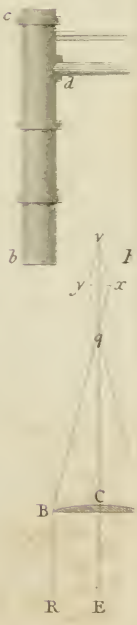

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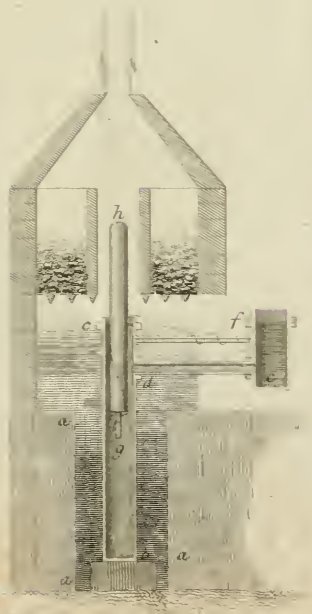
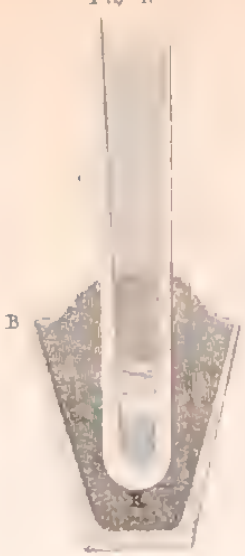
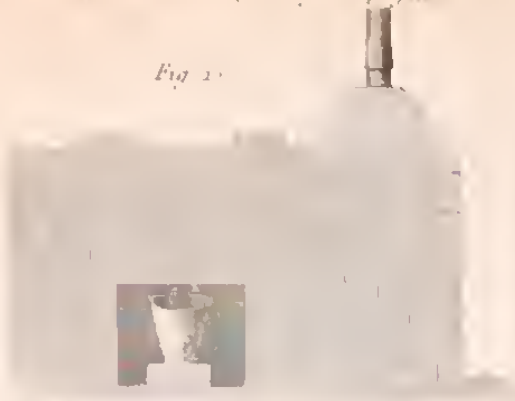
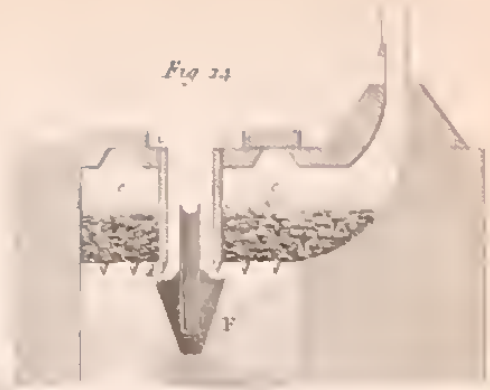
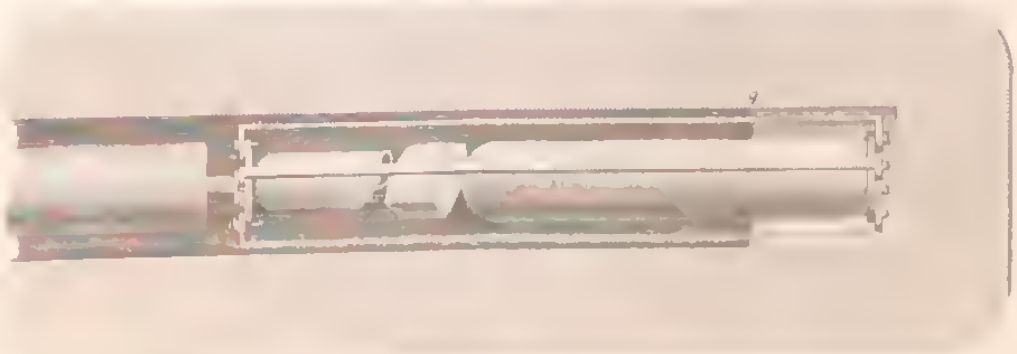




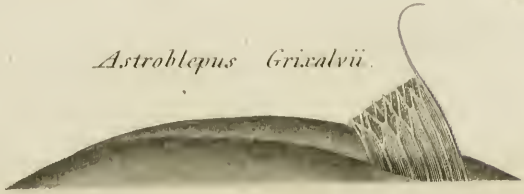




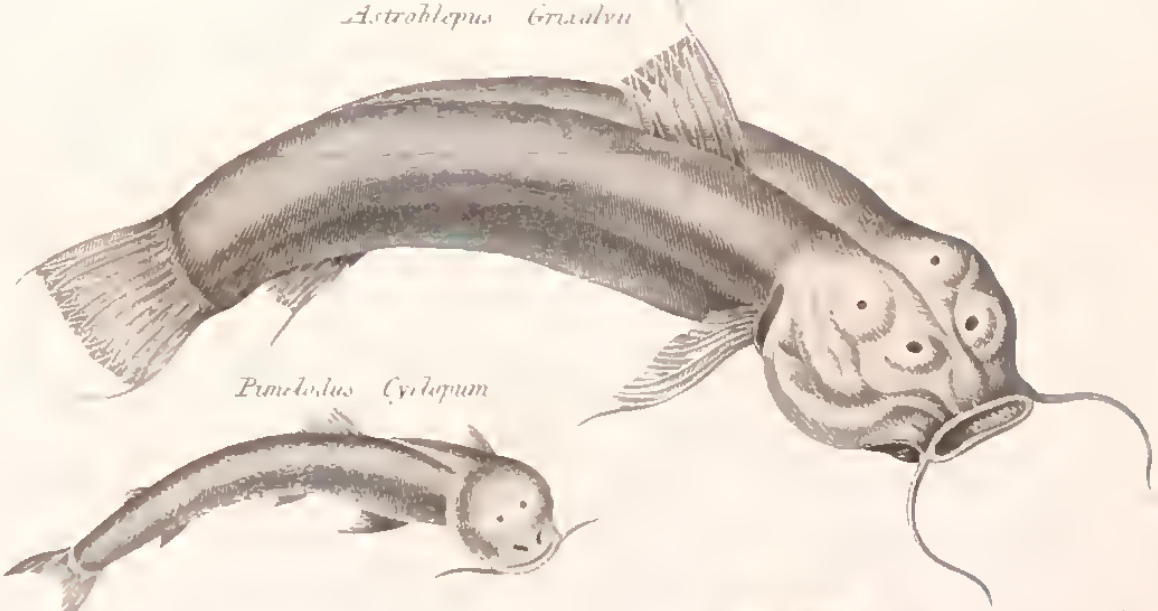


Eremophilus Mutisii.

Astroblepus Grixalvii.



Astroblepus Grixalvii



Pimelodus Cyclopum

Simia Leonina

R

E

Fig 2

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For May 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. 11 o'Clock, Night.	Thermometer. Noon.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
April 27	45°	52°	43°	30·10	22°	Cloudy
28	42	47	40	29·78	25	
29	43	55	45	·56	35	Cloudy
30	46	54	46	·67	28	Showery
May 1	50	56	51	·80	29	Cloudy
2	54	63	53	·60	48	Fair
3	48	46	41	·78	0	Rain
4	45	46	44	30·06	17	Rain
5	44	52	46	·02	5	Rain
6	50	64	55	29·78	30	Fair
7	55	68	55	·59	36	Fair
8	56	69	56	·56	45	Fair
9	54	63	56	·50	15	Cloudy
10	55	67	55	·65	60	Fair
11	50	62	55	·80	11	Cloudy
12	50	59	45	·75	27	Fair
13	45	51	48	·70	5	Cloudy
14	50	60	52	·40	0	Rain
15	55	62	53	·59	42	Fair.
16	55	66	54	·94	51	Fair
17	54	65	55	30·02	41	Fair
18	58	70	54	·30	70	Fair
19	56	65	51	·35	65	Fair
20	52	69	50	·20	66	Fair
22	51	48	68	·18	50	Fair
23	50	62	51	·20	60	Fair
24	54	71	52	·02	62	Fair
25	56	73	61	29·99	57	Fair
26	60	73	59	·90	71	Fair

N. B. The barometer's height is taken at noon.

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ERRATA.

Page 166, line 10, for "the solution," &c. read "the silver, again dissolved in nitric acid and diluted," &c.—Page 167, l. 18, for "CIVNI," read "CIONI."—Page 183, l. 18, for "tension," read "charge."

The plate of Mr. Steevens's Gasometer, instead of being numbered Pl. III. should be Pl. V.

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